SETTING PRIORITIES IN SCIENCE

HEARINGS

BEFORE THE

SUBCOMMITTEE ON SCIENCE

OF THE

COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

U.S. HOUSE OF REPRESENTATIVES

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WITNESSES

SETTING PRIORITIES IN SCIENCE

TUESDAY, APRIL 7, 1992

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
SUBCOMMITTEE ON SCIENCE,
Washington, D.C.

The subcommittee met, pursuant to call, at 10:00 a.m., in Room 2325, Rayburn House Office Building, Hon. Rick Boucher [chairman of the subcommittee] presiding.

Mr. KOPETSKI. (Presiding.) The subcommittee will come to order.

This morning Chairman Boucher has been delayed, so we're going to begin this hearing without him. I'm certain he'll be joining us shortly.

This is the first of a series of two hearings on the process of priority setting for federally-funded research. This set of hearings is designed to assess the effectiveness of the current process and develop recommendations for improvement. As stated in a 1991 Office of Technology Assessment report, the renewed interest in this issue stems in part from concerns over the growing mismatch between research needs and opportunities and the capacity of the public sector to provide the necessary resources, limited growth in the availability of research funds as a result of the budget deficit, and the need to ensure that Federal investments in research contribute to national goals.

Today's hearing will examine the adequacy of the current process of setting priorities in the Federal funding of research by looking at examples of priority setting activities carried out by Federal agencies, the research community, and the Federal Coordinating Council for Science, Engineering and Technology, as well as the process of the Office of Science and Technology Policy.

Before we go to the witnesses, I believe some of the Members have opening statements. Mr. Packard, good morning.

Mr. PACKARD. Thank you very much, Mr. Chairman.

I would like to take this opportunity to welcome the witnesses, all of the witnesses who will be testifying today. In our first panel we have Dr. Massey and Dr. Powell, who have been before us, and we're grateful to see them, and then a special welcome to Dr. Healy, who is here, I think, for the first time, and we're grateful that you've come. We appreciate each of you coming to share your insights and hope that this first in a series of hearings will lay the groundwork for some concrete steps in the direction of prioritizing scientific projects.

This is a critical issue that we face today and well into the future. Why? Because there are always more research opportunities than there are research dollars. This gap between resources and demand will most likely remain as long as we can see into the future.

Therefore, it is essential that the scientific community, the universities, Federal agencies and Congress work together to improve the current system of setting priorities in research funding. I don't believe that there is any disagreement that scientific merit and mission relevance should remain the principal criteria; however, we need to look at what other criteria are necessary to rank projects.

My thanks to Chairman Boucher, who has yet to come, for calling this series of hearings, especially in the way in which this hearing is structured. I think it will be particularly helpful to this subcommittee to hear about priority settings by our Federal agencies, the research community, and the Federal Coordinating Council for Science, Engineering and Technology. We're very grateful again to have the witnesses here and we look forward to your testimony.

Thank you, Mr. Chairman.

Mr. KOPETSKI. You're welcome. Thank you, Mr. Packard, for that fine statement.

Mr. Schiff, Good morning.

Mr. SCHIFF. Good morning, Mr. Chairman.

Very briefly, I would like to say two things. The first is I want to congratulate Chairman Boucher and our ranking Member, Congressman Packard, for scheduling these hearings. We all know that we are in a period, and will continue to be in a period of budget constraint and, therefore, it's especially important that we examine carefully what are our priorities in scientific research and how do they fit with the other priorities that the Nation is establishing as we go into a new era from the cold war.

Second of all, I want to especially welcome one of the witnesses on the second panel, Dr. Paul Risser, who is vice president and provost of the University of New Mexico. A provost, I'm told, means he's in charge of a little bit of everything that happens at the university.

Since I became a Member of Congress in 1989, Dr. Risser has been in charge in that institution of research projects, and I am very, very pleased that the committee selected him to testify.

I have to add to Dr. Risser and the Chair and the other witnesses that I'll be doing a bit of back and forth, because I'm also involved with a hearing beginning at this time on the efficacy of the Patriot missile in last year's Gulf war that I'm also attending. So, with that, I will say I appreciate the fact that this hearing is being scheduled.

Thank you.

Mr. KOPETSKI. I appreciate Mr. Schiff's statement.

Mr. Boehlert, good morning.

Mr. BOEHLERT. Thank you, Mr. Chairman.

I feel like a nonswimmer thrown in the middle of the ocean, coming up for the third time. I'm screaming "Help". I think Congress needs help in setting science priorities. We don't do a very good job of it. Quite frankly, what we do is just approve everything that's sounds good and never really look back over our shoulder to see what should be continued and what should be scrapped. As a result of that, we have a four trillion dollar national debt and we're spending about $741 million a day, every 24 hours, in interest on that debt. Now, that doesn't advance anybody's scientific interest. So I'm reaching out to you and saying help us, because we're not doing very well all by ourselves.

It's mindboggling for me, for example, to think in terms of the superconducting supercollider being given a very high priority, $650 million alone in this year's budget, and that's just one installment for something that's going to cost about 300 percent more than originally projected. That bothers me when I think of you, Dr. Massey, and you, Dr. Healy, getting those applications from all those bright people across the country, that want a $100,000 principal investigator grant or something, to explore some new area of science that promises a great return for the Nation. I sort of wonder where our priorities are when we say no to all those scientists for their little projects and we say yes to this thing that no one can quite figure out yet. So we've got a lot to do in terms of science priorities.

It bothers me to think of our competitive position in the 21st century, Mr. Chairman, which is less than a hundred months away, when we have our youngsters in public schools in America, more than 50 percent of them are being taught science by people not certified to teach science. They may be French majors, and I have nothing against French majors or history majors. But they're not science majors.

So I wonder where we're going as a nation, and I'm looking to you for some guidance and for some direction. I'm yelling help from the Congress because we need the help. We've got to do a better job of establishing some priorities so that our precious and limited resources are channeled in the right direction, to guarantee America's preeminent position in a very competitive global marketplace. So I couldn't be happier than I am today to see you here, the three of you, and I look forward with a great degree of interest in your testimony.

I must confess I am very disappointed to look at this side of the table. We've got four Members of Congress. The place should be packed. This is serious business.

Thank you, Mr. Chairman.

Mr. KOPETSKI. Thank you, Mr. Boehlert.

I think the Members have done a tremendous job in framing the questions and some of the ramifications of not having an identified policy in this arena. I'm sure that the panel's testimony is going to be instructive and help us move in a more positive and cost-effective direction.

Without objection, I will ask the Members to have the full statements of each of our witnesses on this first panel entered into the record, and ask that—normally our rules are such that we ask the witnesses to summarize their testimony in five minutes. Today, though, we would appreciate it if you would take a little bit longer, around ten minutes, to summarize your testimony and present a more complete picture for us of some of your thoughts in this area.

[The prepared statements of Messrs. Boucher and Packard follows:]

OPENING STATEMENT OF THE HONORABLE RICK BOUCHER, (D-VA) CHAIRMAN, SUBCOMMITTEE ON SCIENCE ON PRIORITY SETTING IN SCIENCE

April 7, 1992

This morning the Subcommittee on Science begins a series of three hearings on the crucial issue of setting priorities for the funding of science research by the Federal Government.

The 1991 Office of Technology Assessment report Federally Funded Research: Decisions for a Decade, points out that there will always be more good research opportunities than the government through its resources can fund. There will always be more requests for federal support for worthwhile projects than the federal research budget can meet.

Due to large increases in the ranks of researchers, we confront a situation today in which the total number of faculty with the preparation for and interest in research surpasses the capacity of the system to support them.

No longer is there an assurance that a grant proposal for an obviously worthwhile project will receive funding. It must now compete with other admittedly meritorious proposals.

While the federal civilian research budget is growing significantly, our dollars are simply not keeping up with the demand.

We are, therefore, faced with the reality that while more projects than ever before are being funded, there are also more good proposals than ever before that are not.

This circumstance has led to profound discouragement among the ranks of researchers. Some say it threatens the stability of the research enterprise.

The problem cannot be successfully addressed merely by increasing research budgets. The time has arrived when hard choices must be made, and priorities must be set.

The goal is easy to state but difficult to achieve. Decisions must be made across agency budgets, weighing the relative value of megascience projects and small science basic research, deciding within disciplines which goals have the greatest immediacy and which can be deferred, and establishing coordination within the Congressional structure which is characterized by fractured research jurisdiction among numerous committees of both the House and Senate.

Nevertheless, it is a goal we must achieve, and we begin the process of examining the means of achieving it this morning. The hearing will focus on the roles and responsibilities of federal agencies, the research community, the National Science Board, and the Federal Coordinating Council for Science, Engineering and Technology. In later hearings, we will review the specific role of the Office of Science and Technology Policy (OSTP) and the Office of Management and Budget (OMB) in setting priorities for federal research funding.

STATEMENT OF
THE HONORABLE RON PACKARD
SCIENCE SUBCOMMITTEE
HEARING ON SETTING PRIORITIES
9:30 A.M., 2325 RHOB
APRIL 7, 1992

Thank you Mr. Chairman,

I would like to welcome all the witnesses who will be testifying today -- Dr. Massey and Dr. Powell -- and a special welcome to Dr. Healy who does not regularly appear before this subcommittee. We appreciate you coming to share your insights and hope that this first in a series of hearings will lay the groundwork for some concrete steps in the direction of prioritizing scientific projects.

This is a critical issue that we face today and well into the future. Why? Because there will always be more research opportunities than we are able to support. This gap between resources and demand will most likely remain for as long as we can see into the future.

Therefore, it is essential that the scientific community, universities, Federal agencies, and Congress work together to improve the current system for setting priorities in research funding. I don't believe that there is any disagreement that scientific merit and mission relevance should remain the principal criteria, however, we need to look at what other criteria are necessary to rank projects.

My thanks goes to Chairman Boucher for calling this timely hearing and especially for the way in which this hearing is structured. I think it will be particularly helpful to this subcommittee to hear about priority setting by: Federal agencies, the research community, and the Federal Coordinating Council for Science, Engineering, and Technology.

Mr. KOPETSKI. To begin today's witnesses, we have with us Dr. Bernadine Healy. Dr. Healy is the Director of the National Institutes of Health, located in Bethesda, MD. I welcome you this morning to our hearing.

STATEMENTS OF BERNADINE HEALY, M.D., DIRECTOR, NATIONAL INSTITUTES OF HEALTH, DEPARTMENT OF HEALTH AND HUMAN SERVICES; WALTER MASSEY, DIRECTOR, NATIONAL SCIENCE FOUNDATION; AND JAMES POWELL, MEMBER, NATIONAL SCIENCE BOARD

Dr. HEALY. Thank you, Mr. Chairman and members of the subcommittee. I am pleased to have the opportunity to talk with you about the strategic plan currently being developed by the NIH. As I'm sure you know, the NIH has a big responsibility; namely, generating new scientific and medical knowledge to improve this Nation's health. And when I speak of the Nation's health, I mean not only the health of every man, woman and child in this country, but also the health of the Nation's economy. That's a heavy responsibility, and one that requires foresight, commitment, and creativity.

It is a responsibility that reminds me of something said many years ago by Daniel Burnham, the man who developed the master plan for the City of Chicago back in 1909. He said, "Make no little plans: they have no magic to stir men's blood." Little plans don't stir women's blood, either, nor do they serve institutions well.

About a year ago, the NIH began making some plans. We embarked upon a process designed to transcend immediate concerns and ensure the future strength and vigor of biomedical research. We had decided that, in order to advance our large and complex enterprise, we must plan beyond the next budget year. The purpose of our strategic planning is not only to achieve predictability and stability, but also to capitalize on the extraordinary opportunities in burgeoning areas of life sciences and medicine.

Our proposed strategy not only reflects the commitment and cooperation of all 20 of our institutes, centers and divisions of NIH, but also the biomedical research community. Although a new undertaking for our agency, the strategic plan does not sever ties with the past. Instead, it builds on our past accomplishments, organizational strengths, and mechanisms and approaches of proven value. Our draft plan is not a grand design that imposes rigid timetables or predictions about the future. Rather, the process creates a framework for our cohesive thinking and for charting a course that will prepare us for the future. Above all, our proposed strategic plan must be a reflection of NIH's "ordo amorum"—an idea expressed by St. Augustine—the order of our loves or, in short, our priorities.

Before going into more detail about where we are in the development of the strategic plan, it is important to acknowledge that planning and priority setting are not altogether new for NIH. Key strategic decisions formulated in the 1950s by Vannevar Bush and the Office of Scientific Research and Development have shaped our research and are largely responsible for the best of what NIH is today. Let me mention three key issues.

First, it was decided back then in the Fifties that federally-supported biomedical research would exist as a separate agency under the umbrella of the Public Health Service and not as a component of the National Science Foundation. This strategy has served NIH, and I believe the Nation, very well, and on a parochial level, probably Dr. Massey as well.

Second, that investigator-initiated research would be supported through grants to universities and research institutions, chosen by merit through a system of peer review.

And third, that the NIH would be the National Institutes, plural, of Health, and its strategic components, the institutes, for the most part, would be disease-driven.

Those decisions guided the growth and development of the NIH from the 1950s to the 1990s. But times are now different and demand another look. For one thing, a revolution in biology has transformed medical research. We see before us a field of molecular medicine unimaginable 50 years ago. Molecular medicine reflects this Nation's exploration of "inner space"—the cells, the genes, the molecular structure of the human body. Molecular medicine encompasses structural biology, biotechnology, human genome exploration, gene therapy, vaccine development, and contributes to our fundamental understanding of virtually all human disease, including Alzheimer's disease, cancer, mental illness, and drug and alcohol abuse.

Another change over the past 40 years is that the practice of science has become eminently more complex, expensive, interdisciplinary, and rapid. Today, the challenge is to advance technologies critical to our future, yet at the same time ensure that individual creativity and imagination of individual scientists will not just survive but flourish, and that institutions that support that creativity and imagination also flourish.

Mr. Chairman, NIH is now a $9 billion public enterprise with 20 institutes, divisions, and centers. We have close to 15,000 Federal employees, 194 chartered advisory committees, more than 3,400 consultants, and partially or totally support an estimated 100-150,000 people through our grants. We have five Federal facilities outside of Bethesda, and provide grants and contracts to more than 1,800 institutions, including 500 small businesses. The NIH currently supports more training and career development in science than any other Federal agency, and we are the largest supporter of biomedical research in the world.

Most important of all, NIH's research portfolio can be viewed as one of the keystones of our national domestic security, essential to America's capacity to respond to the health needs of her people. Today, this capacity is large, diversified, successful, and gives the American public extraordinarily high expectations that NIH can and will respond quickly to virtually all health problems. A strong NIH-supported research base, the ability of entrepreneurs to finance new ideas, and the industrial capacity to convert basic science into products are key elements in ensuring our Nation's future domestic national security, as well as global competitiveness in biotechnology, as we enter this next century.

This is a time when excellence in management of NIH resources is critical to our success, as is scientific excellence. As we enter the last decade of this century, many of us wonder what lies ahead for our Nation and for biomedical research. Let me remind you of what the American philosopher Eric Hoffer wrote in his book, *The Passionate State of Mind.* He said, "The only way to predict the future is to have power to shape the future."

Strategic planning is all about an organization's participating in the shaping of our future. In our planning process, we are building on time-honored strategies and mechanisms, but we are also looking at new dimensions that focus not just on disease but on prevention, nutrition, and behavior.

In initiating our strategic planning process, two important principles have guided our efforts. First, there will be no finality to the strategic planning. It must be an ongoing, living, breathing, growing process. This process must be capable of rapidly accommodating new scientific opportunity and responding to emerging health emergencies. Second, the plan is not to be a rigid blueprint; rather, it will serve as a compass to guide us in our course of discovery.

In the course of our planning, we have identified five trans-NIH objectives which have been considered by the extramural community in a series of five public meetings held over these past few months across the country.

The first objective is to ensure that critical science and technologies in basic biology and the other sciences are advanced as priorities across the NIH. Investments in critical areas of science and technology will set the stage for improving health, reducing health care costs, and bolstering this Nation's economic well-being.

The second objective is to strengthen the capacity of our Nation's biomedical and behavioral enterprise to respond to current and emerging public health needs. The individual institutes with their focus on disease and human health are central to this objective.

The third objective is to provide for the renewal and growth of the intellectual capital base essential to biomedical research. We can only be as creative and successful as the scientists who make up our enterprise. Ensuring fairness and equity of opportunity at NIH is also central to our efforts to enhance the human resource base of medical research.

The fourth objective is to secure the maximum return on the public's investment in our enterprise. Stewardship of public resources requires that we ensure efficient and responsible managers, quality management systems, and integrity and fairness in the conduct of our business.

And finally, the fifth objective is to earn continually the public's respect, trust and confidence as we carry out our noble mission. Although the NIH ranks among the top three most respected government agencies, this respect cannot be taken for granted. As a public enterprise that is of vital importance to the lives of every man, woman and child in this country, we must hold ourselves to the highest standards.

We are now at the critical juncture of providing for implementation. The NIH's institutes, centers and divisions will be key to the success of our strategic planning, for they will be agents for implementation. And implementation will complement and make use of the existing planning mechanisms within our institutes.

The institutes will be responsible for implementing specific initiatives, virtually all of which will be pertinent to their individual research missions, and at a broader level, we want our budget to be driven by the strategic plan. In the past, the budget has generally focused on mechanisms rather than on cross-cutting opportunities in science. In the future, we will cast our budget in terms of scientific programs and not mechanisms.

In implementing the plan, we will continue to reaffirm the principle of high quality, investigator-initiated research, which has been the hallmark and the success of NIH. Also, we are committed to maintaining the balance and diversity of our research portfolio at the same time that we emphasize certain areas of opportunity, such as molecular medicine.

We are relying on the extramural community to play a continuing role in the development of our plan. With the completion of the regional meetings, our next step is to hold a national task force meeting of 150-200 extramural scientists. In June, panels composed of key non-Federal scientists will meet to review, with NIH leadership, issues relating to the five objectives that I have just outlined. The panels will also focus on training and infrastructure issues, cost management, peer review, and the development of a scientific code of ethics.

Following the task force meetings, a third Director's retreat will be held to develop an overarching summary of issues raised in all of these earlier meetings. This retreat will involve members of the extramural scientific community, lay advisors representing the public, the NIH leadership, as well as representatives from other constituency groups.

In conclusion, Mr. Chairman, through our strategic plan, we are creating the NIH of tomorrow and hoping to shape the future of biomedical research. We have invited the public research community and the American public to participate in our planning, because we recognize the impact that the NIH has on their lives. We also recognize that the opportunities for major achievements in biomedicine and the life sciences have never been greater, and we want to ensure that the United States will be in a position to take full advantage of these opportunities.

Thirty years ago, President Kennedy pointed out that "...those who look only to the past or the present are certain to miss the future." The strategic plan will be NIH's window to the future, a means by which we can more effectively address future challenges and sustain and strengthen biomedical research and our Nation's economy. We fully recognize that the ultimate beneficiaries of our efforts must be the American people. We look forward to working with you and with other Members of Congress as we try to create the NIH of tomorrow.

Thank you.

[The prepared statement of Bernadine Healy follows:]

FOR RELEASE UPON DELIVERY

STATEMENT BY

BERNADINE HEALY, M.D.

DIRECTOR

NATIONAL INSTITUTES OF HEALTH

DEPARTMENT OF HEALTH AND HUMAN SERVICES

BEFORE THE

HOUSE SCIENCE, SPACE AND TECHNOLOGY

SUBCOMMITTEE

ON

SCIENCE

APRIL 7, 1992

Mr. Chairman and Members of the Subcommittee, I appreciate the opportunity to appear before you to discuss the future of the biomedical research enterprise, its relationship to the health of our people, and the NIH strategic planning process that will help guide U.S. medicine into the 21st Century.

As the next century approaches, we must pause to consider the enormous potential of biomedical research and ask ourselves whether the American biological research enterprise is poised to meet the challenges of the future. I would like to explore with you the role of NIH in addressing the extraordinary opportunities such challenges imply, through the development of our *Framework for Discussion of Strategies for NIH*, an ongoing planning process we expect to complete this year.

No nation can be prepared to meet its challenges and provide "domestic national security" without a healthy population and a plan for its future research endeavors. The NIH plays a major role in ensuring the health of the nation's people through the development of diagnostics, therapies, and interventions that prevent disease and reduce suffering from disabilities.

One year ago, we embarked upon a process to take the steps necessary for the biomedical research community to confront the challenges of the future. To advance this sophisticated and complex enterprise, it is imperative that we plan beyond the next budget year. The purpose of planning is to achieve predictability and stability, which will allow us to capitalize on the opportunities in the burgeoning areas of biology and medicine.

Our framework for discussion is the product of the reflection, commitment, and cooperation of all the Institutes, Centers, and Divisions (ICDs) that comprise the NIH, as well as the extramural community. The intent is to join forward thinking with historical strengths: accomplishments, organization, mechanisms, and approaches of proven value. The framework is not a grand design that imposes rigid timetables or predictions about the future--rather, it is a process for our corporate thinking and for charting a course that will prepare NIH for the future.

It is important to acknowledge that planning and priority setting are not new to the NIH. Furthermore, our success today is the result of the sound strategic planning from earlier times. Key strategic decisions made in the post World War II era did much to forge the research enterprise and are largely responsible for the best of what NIH is today. I would like to briefly describe those decisions.

First, there was the preparation of *Science -- the Endless Frontier*, which laid out a blueprint for Federal research that is still followed today. This milestone report by Presidential Advisor Vannevar Bush to President Truman, led to the creation of the National Science Foundation, and his inquiries led to the strengthening of the National Institute of Health within the U.S. Public Health Service.

Second, was the formulation of the process that investigator-initiated research would be financially supported by NIH grants to universities and other research institutions. In the post World War II economy, surplus dollars remained from military research contracts. NIH staff asked all U.S. medical schools if they needed additional financial support. Subsequently, a two-tiered system was created to review applications for grants-in-aid developed by NIH personnel, now known as the "peer review" system. This system supports investigator-initiated research through such grants awarded on the basis of scientific merit.

Third, in 1948, reorganization changed the concept of the NIH. At the beginning of World War II, the National Institute of Health was composed of eight divisions and the National Cancer Institute. In 1948, the creation of the National Heart Institute made NIH the "National Institute<u>s</u> of Health." Through plans for a revised NIH, put forth by citizens with a strong interest in health, such as, Mary Lasker and Frances Mahoney, and Members of Congress, such as Senators Claude Pepper and Lister Hill, and Congressman John Fogarty, NIH shifted its focus and organizational composition we recognize today.

The utility of the Vannevar Bush plan was proven by its lifespan of over 40 years. The plan marked the first time any nation looked comprehensively at its research enterprise beyond its wartime necessity. We built over these 40 years on this foundation, ensuring a stable, Federally-supported biomedical research system. That stage has now been completed. The advent of advanced technologies, such as the use of recombinant DNA and other molecular analyses, have ushered in a new era that has begun to blur the traditionally understood distinctions between disciplines. For example, basic research on cellular growth has shown us that there is an interrelatedness between the processes of uncontrolled proliferation--cancer and terminal differentiation leading to senescence--aging. As we have begun to understand these fundamental aspects of biology, we have recognized that our old approach has been a vertical growth of disease-focused disciplines. In response to what has occurred in the life sciences, and in response to what has occurred in the broader environment around us, we must now examine a horizontal, transcendent plan that will integrate these previously compartmentalized orientations.

The Changing Face of NIH

As I have illustrated, this era in biomedical research is one of unprecedented opportunity, a powerful new age of discovery about living organisms and the application of these discoveries to human health. Our challenge is to advance the technologies critical to the future, to be attentive to infrastructure and technical needs at the pace that accompanies them, and to continue nurturing and encouraging individual creativity.

We are now a $9 billion public enterprise with 20 institutes, divisions, and centers. NIH has close to 15,000 Federal employees and 194 chartered advisory committees with more than 3,400 consultants. We, also partially or totally, support an estimated 100,000 people through grants and other funding mechanisms. We have five Federal facilities outside the Bethesda campus, and grants and contracts are provided to more than 1,800 institutions, including 500 small businesses.

NIH grants average approximately $250,000, with some far exceeding $1 million per year. These grants cover a broader spectrum of science, and as a result, approximately 140 study sections encompassing more than 80 disciplines of science, have been established to guide their review. What this means to me is that more than ever, we must critically examine how we are managing ourselves and our resources. This is a time when excellence in management of our resources is as crucial to the success of NIH as is scientific excellence. It is a time when public confidence and public trust have never been more important.

Strategic Planning--An Evolving Process

Our agency is participating in strategic planning to prepare itself for what lies beyond the horizon. Building on time-honored strategies is wise, but we are also challenged to look at other dimensions--those that focus not just on disease but on health--prevention, nutrition, and behavior. These dimensions demonstrate: the fundamental unity of basic biology and molecular genetics; the critical impact the results of NIH research have on the Nation's economy; and, the social, legal, and ethical issues that arise in the context of modern biology and medical research.

Throughout this strategic planning process, we have been guided by the principle of flexibility. There should be no end to the effort, rather it must remain part of the plan itself, updated and revised as warranted. This process must be capable of rapidly accommodating and responding to both new scientific opportunities and emerging public health problems. It must reflect the will and the resolve of the community including the various scientific groups and research institutions, but most important, our ultimate community, the public at large.

Public Health Service and Departmental health goals, such as those enunciated in Healthy People 2000 and in the Secretary's program directions, are factors in our planning decisions. Congressional mandates and annual appropriations decisions clearly influence the programmatic focus and direction of our research. The National Advisory Councils, through their public advisory functions and reviewing of research proposals for program relevance, are also an important public factor in program planning and priority setting at the NIH. In addition, the framework is consistent with, and will further the scientific priorities of the Administration, including biotechnology and high-performance computing. Also, the goal of the framework is to complement the public health mission of the other agencies within the Public Health Service.

In developing our strategic plan, we had to reassess the blueprint of 1948, which provided structure and organization. We overlaid a framework on this foundation for the development of a transcendent approach to biomedical science and a forum for the discussion of science policy issues. In brief, our draft mission statement reflects the heart and soul of our institution and underscores our concern for the public health to pursue new knowledge to extend healthy life, and reduce the burdens of illness and disability. We are more than a science agency, more than a public health agency, we share with the American public an abiding commitment to the importance of human health and the enhancement of the quality of life for everyone.

To advance that mission, we identified five trans-NIH objectives. As we look ahead, success in pursuit of each of these objectives is essential to being true to our mission:

1. <u>Critical Science and Technology</u>--To assure that critical science and technology in basic biology impacting on human health and the national economy are priorities across the NIH. Investments in critical science and technologies in basic biology will set the stage for improving health, reduce health care costs, and bolster the nation's economic well-being. The operational components of the framework that relate to and advance this objective are: molecular medicine, biotechnology, vaccine development, and structural biology. These areas transcend the categorical institute missions and contribute substantially to the understanding of most diseases and the enhancement of the nation's economic growth, productivity and competitiveness.

2. <u>Research Capacity</u>--To strengthen the capacity of the national biomedical and behavioral research enterprise to respond to current and emerging public health needs. Central to achieving this objective are the institutes and centers with their focus on specific diseases and human health and the robust and diverse network of research institutions that reside in communities throughout the country, as well as, the intellectual and physical infrastructure of that research capacity. A strong research capacity ensures that the public health needs of today are addressed, and that there is advancement in disease prevention and the promotion of the quality of life for Americans.

3. <u>Intellectual Capital</u>--To provide for the renewal and growth of the intellectual capital base essential to the biomedical research enterprise. Ensuring fairness and equality of opportunity is central to efforts to enhance the human resource base of biomedical research. The NIH supports more

training and career development in science than any other Federal agency. Increasing the number of well-trained biomedical and behavioral scientists is critical to maintaining and enhancing the talent pool of science, including strengthening research training, and the recruitment and retention of underrepresented groups in science. This objective is vital not only to the future of NIH, but to the biotechnology industry.

4. Stewardship of Public Resources--To secure the maximal return on the public investment in the enterprise. The NIH must maintain a diversified basic and clinical research portfolio, managed by our research components, emphasizing investigator-initiated research and quality management systems. Stewardship of public resources also entails the support of outstanding scientists, attention to the research resources, and improvement of the physical infrastructure, including instrumentation, equipment, and facilities. It also requires that we ensure innovative and responsible management, as well as integrity and fairness in the conduct of our daily work.

5. Public Trust--To continually earn the public's respect, trust, and confidence as we carry out our mission. The NIH has consistently ranked among the most respected governmental agencies, and this achievement must not be taken for granted, particularly in these times. As a public enterprise that is of vital importance to every man, woman, and child, we must hold ourselves to the highest standard. The advances in biomedical research affect individuals in the most intimate and personal ways. What we learn, we do so with the objective of improving the quality and span of life. The rapid progress and growing complexity of science and the public's heightened

expectations for the research enterprise necessitate closer attention to the social, legal, and ethical issues inherent in biomedical and behavioral research; professional standards of science; efficient communication of facts to the public; the impact of research on health care; and the transfer of technology to foster collaborative endeavors in the public interest.

It is important to emphasize that the NIH strategic plan will not involve micromanagement of research projects or efforts to specify the details of research, nor will it interrupt the ongoing activities or discrete missions of the individual institutes. In addition, no strategic plan can predict the full range of issues that are of importance to future development. The path of scientific discovery cannot be defined in advance. However, the NIH believes that success in pursuit of our mission can be catalyzed and accelerated by judicious planning. Research opportunities and important issues of science policy can be identified and advanced to achieve NIH's long-term goals.

<u>Role of the Extramural Community in the Development of an NIH Strategic Plan</u>

Although NIH leadership took the first steps to develop the strategic planning process, it was recognized that essential to its success was involving the extramural community in the effort. The public and the research community have been, and will continue to be, party to the development of the plan. This is critical because of the diverse constituencies we serve and depend upon, the intrinsic mutability of scientific discovery, and the ever-increasing pace of change in the health sciences. Involvement of members of the public and scientists outside of the NIH is the only way for NIH strategic planning to work.

We at NIH sought advice from our National Advisory Councils and interested professional and scientific groups very early in the development process, and then established a mechanism for soliciting formal public input through a national symposium and a series of four regional meetings around the country in Los Angeles, California; Farmington, Connecticut; Atlanta, Georgia; and St. Louis, Missouri. We notified the public of these meetings through widely disseminated announcements and by sending letters of notification to more than 10,000 investigators, voluntary health organizations, and scientific societies across the country. Plans for the regional meetings were extensively reported by the media as well. We were very pleased with the extent of participation in these five meetings, with over 1,100 people in attendance.

We are now in the process of reviewing and analyzing the recommendations received during the deliberations, and from the over 170 written statements and letters that were submitted by the public and scientific community, many of which were unable to attend a regional meeting. I would like to share with you some of the major themes that emerged from these activities:

- Despite initial skepticism from many sectors, the concept of strategic planning for the NIH was strongly endorsed by the extramural community. There was wide recognition of the need for a focus on the future of biomedical research and the agency's ability to carry out its mission into the next century.

- The fundamental role of investigator-initiated research in the advancement of health science research was reaffirmed. NIH's continued commitment to the ideas and contributions of individual scientists was regarded as imperative.

- The importance of peer review was also emphasized. The strategic planning process was identified as a mechanism for undertaking a comprehensive review of the current peer review system to ensure that study sections keep pace with advancements in science.

- Meeting participants also identified the critical importance of fundamental research and its relationship to more directed program activities, as well as the future of the enterprise.

- The importance of expanding public-private partnerships in the biomedical research enterprise was highlighted. The NIH was encouraged to investigate ways to expand the degree of participation by biotechnology, pharmaceutical industries and philanthropic institutions.

- The education and training of the scientists of the future was viewed both as a short and long-range priority. The ability of the NIH to carry out its mission is vitally dependent on sustaining and enlarging the flow of talent into science.

- Extramural infrastructure needs are significant and a strategic plan should address resource allocation issues relative to facilities and instrumentation needs.

We are relying on the extramural community to play a continuing role in development of the plan. With the completion of the regional meetings, our next step is to hold a National Task Force Meeting of about 150-200 extramural community members in June of this year. At that time, panels composed of key non-Federal scientists will meet to review issues relating to the five objectives proposed in the *Framework for Discussion of Strategies for NIH*.

Subsequent to the task force meeting, a third Director's Retreat will be held to synthesize an overarching summary of issues raised by the NIH, other components of the Department of Health and Human Services, the National Advisory Councils, the national symposium and regional meetings, and the National Task Force. This retreat will be a joint effort involving members of the extramural scientific community and lay advisors, as well as the NIH leadership, and will be open to the public.

Priority Setting Criteria Emanating from the NIH Strategic Plan

Setting priorities, inherent in the process of developing a framework, is grounded in our mission, goals, objectives, and implementation principles. Criteria, used in identifying priorities in both science and policy, encompass the range of biomedical and behavioral disciplines and diseases.

We propose critical science and technology as a priority because it has the potential to transform medicine as we know it today. It can provide precise biological interventions whereby cellular and molecular targets are identified for preventive and therapeutic responses. The ideas and discoveries that emanate from molecular medicine embrace a broad spectrum of disciplines: molecular biology and cell signalling; structural biology and rational drug design; molecular genetics of living organisms; vaccine development not just for infectious diseases, but for cancers and chronic debilitating illnesses; and the development of novel bioengineered products. As such, critical science and technology discoveries not only offer the long-term promise of containing and reducing health care costs, but also offer substantial contributions to the enhancement of the nation's future economic growth, productivity and competitiveness.

Plans for Implementation

We are now at the critical stage of formulating the implementation of the plan. We expect to define specific targets and goals toward achieving NIH's long term objectives. These, too, must be developed in conjunction with the broader community. The NIH's research components will be key to the success of our strategic plan, and will serve as the agents for implementation. The ICDs will be responsible for implementing specific initiatives, virtually all of which will be pertinent to their individual research missions. Individual project decisions will be made by the ICDs, and in many cases, there will be collaboration of their initiatives. Coordination of these individual efforts will be conducted within the Office of the NIH Director.

Conclusion

In conclusion, Mr. Chairman, we believe that the opportunities for major achievements in biomedicine and the life sciences have never been greater. For us, the process of viewing the NIH in its strategic context will provide a means to more aggressively address future challenges and strengthen our vital enterprise. We look forward to working with you and other Members of Congress as we continue to develop our strategic plan and address the crucial issues confronting all of us. We look forward to a future of improved health and quality of life for all Americans.

This concludes my prepared statement. I would be pleased to respond to your questions.

DRAFT

FRAMEWORK FOR DISCUSSION OF STRATEGIES FOR NIH

National Institutes of Health

Department of Health and Human Services

January 1992

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A FRAMEWORK FOR DISCUSSION OF STRATEGIES FOR THE NIH

FOREWORD

The National Institutes of Health (NIH) is a national resource engaged in a noble enterprise: improving and safeguarding the health of every man, woman, and child in this country.

NIH's century-long tradition of public service through science began with successful responses to two of the earliest public health challenges facing the one-scientist laboratory that was to evolve into the present-day NIH: the first diagnosis of Asiatic cholera in the United States and the development of the means to increase production of diphtheria antitoxin. Over the years, a fundamental principle that has defined our progress has been the research initiated by creative individual scientists. This approach continues as a major underpinning as the research enterprise. As the principal research arm of the U.S. Public Health Service, Department of Health and Human Services, NIH has become the Nation's and the world's largest sponsor of biomedical research. Furthermore, it has provided sustained support for basic and clinical biomedical investigations that have helped improve the lives of millions and have given hope to millions more. The entire Nation, indeed the entire world, has a stake in NIH's success. Today and for the foreseeable future, that stake is greater than at any time in NIH's existence.

In its many and varied activities, this institution has always worked at the frontiers, building the foundation of knowledge that ultimately leads to improvements in human health and well-being. To advance this large and complex enterprise in the present environment, we must plan beyond the next budget year. Planning, not only to achieve predictability and stability, but also to capitalize on the extraordinary opportunities of burgeoning areas of biology and medicine. Indeed, the most exciting stage of the ongoing revolution in biology is yet to come, when scientists and practitioners widely apply their knowledge to confront disease, sustain health, and, in the process, foster the emergence of a "bioeconomy." Success in this endeavor will enable our Nation to maintain its standing as the acme of biomedical and behavioral sciences and thereby to reap the benefits in both human and economic terms.

Our proposed strategy is the product of the reflection, commitment, and cooperation of all the institutes, centers, and divisions that comprise NIH. Although a new undertaking for NIH, the strategic plan does not sever ties with the past. Instead, it builds on past accomplishments, organizational strengths, and mechanisms and approaches of proven value. Nor is the strategic plan a grand design that imposes rigid timetables or tries to make certain predictions about the future. As some have said, predictions are always difficult, especially about the future.

Rather, this planning process creates a framework for focussing NIH's organizational thinking and charts a course for our efforts which will prepare for and accommodate the future. Most importantly, a strategy for NIH is an expression of our ordo amorum -- an idea expressed by St. Augustine -- the order of our loves, or, in short, our priorities.

We will, of course, alter our course periodically. There will be unexpected breakthroughs - leaps into a new dimension that will create unanticipated opportunities. No plan can predict these developments. A good plan, however, fosters pathbreaking research and enables us to recognize and pursue such surprises. Creation of a stable, flexible, and fair environment for individual pursuit of ideas is inherent in our ability to respond to both the unexpected opportunities and health crises. The pursuit of research opportunities closely aligned with national health goals and public needs is clearly central to our plan. NIH pursues science to advance medicine and health and thereby serve the public -- this is our mission and the top of our priorities.

As the biomedical and behavioral research enterprise prepares to enter the next century, it is timely to recall the genesis of our enterprise as it has evolved these last five decades. In Science: The Endless Frontier, Vannevar Bush, the chief architect of this Nation's ascendancy in science during the post-World War II era, singled out health-related research as a compelling area of science for sustained federal investment. In fulfilling the vision of Vannevar Bush, this Nation created the modern-day NIH, a national treasure with its noble mission. Vannevar Bush's inspiration has guided us for half a century. But, his "endless frontier" has become an expanding universe. The magnitude and importance of our enterprise today calls for a continuation of the foresight and forward planning that has sustained our enterprise thus far.

Our strategy for NIH pledges us and our partners to address opportunities, challenges, and needs of the future with intellectual vigor, dedication, and integrity. In turn, it also calls for a reciprocal commitment from the public and their representatives, not only to sustain, but to enhance the strength of the singular research institution they have created and nurtured. For NIH today to achieve its goals and safeguard the competitive advantages wrought by our "revolution in biology," our noble enterprise must be a priority. Indeed, NIH must be high in the ordo amorum of America.

Bernadine Healy, M.D.
Director, NIH

INTRODUCTION

Overview

The biomedical and behavioral sciences have entered an era of unprecedented opportunity, a new age of discovery and application. Much of this progress is attributable to NIH's sustained support for fundamental biology and clinical research. To ensure that this momentum will go forward and that the past Federal investment in biomedical research will continue to be capitalized, NIH has been engaged in a synergistic process involving all its organizational components, as well as ADAMHA and its research institutes, to develop a framework for discussion of strategies to guide the NIH as it advances into the 21st century. This "framework" identifies activities that we view to be of strategic importance to the success of the enterprise as it impacts on the public's health and the nation's economy into the 21st century. It has a scope that transcends immediate interests and is responsive to changing public and national health needs. Importantly, it builds on past accomplishments, organizational strengths, and mechanisms and approaches of proven value. Finally, it creates a framework for focussing NIH's corporate thinking and charts an initial course for our efforts.

Mission Statement and Goals

As part of this "framework for discussion" we have developed a mission statement, goals, and underlying principles for the agency. These appear in a following section. This unifying corporate philosophy articulates a shared vision of how the biomedical research enterprise and the science it supports will advance.

Trans-NIH Objectives

Anchoring this framework are broad trans-NIH objectives which relate to specific operational components. The objectives are critical science and technology; research capacity; intellectual capital; stewardship of public resources; and, public trust. The operational components include suggested trans-NIH issues in areas of science and policy. While the objectives are inter-related, and therefore cannot be placed in rank order, they represent the context within which the entire framework for discussion should be considered. The objectives and the way in which the operational components relate to them are described in a later section.

<u>**Public Discussion of the "Framework"**</u>

While the NIH is assuming leadership for outlining the framework for discussion, broad public input is being sought and the extramural scientific community is invited to participate in a process that will evolve into a strategic plan. The ICD Advisory Councils and Boards have been involved in this initiative for the past several months and have been asked for their views and advice on elements of the framework related to the areas of science. Broad-based input from the scientific community will be sought through a series of five meetings across the country. The first will be held in San Antonio, Texas, February 2-4, 1992 in conjunction with a national symposium that NIH and the Department of Health and Human Services are cosponsoring with the Southwest Foundation for Biomedical Research. Later in February and March 1992, the NIH will hold four meetings to invite public comment on the proposed framework. Following this wide-ranging review phase, the NIH will develop a strategic plan.

"SCIENCE ADVANCING HEALTH"

NIH MISSION STATEMENT, GOALS, AND PHILOSOPHY

NIH MISSION STATEMENT

Science in pursuit of knowledge to extend healthy life and reduce the burdens of illness and disability

NIH GOALS

1. To foster innovative research strategies designed to advance significantly the Nation's capacity to improve health.

2. To provide the scientific base that will strengthen the Nation's capability to deliver more effective disease prevention and health care in order to enhance the quality of its citizens' lives.

3. To expand the knowledge base in biomedical and behavioral research in order to enhance the Nation's economic competitiveness and ensure a continued high return on the public investment in research.

4. To exemplify and promote public accountability, scientific integrity, and social responsibility.

NIH PHILOSOPHY

Preamble

The NIH is the steward of biomedical and behavioral research for the Nation. Inherent in our mission and goals is a commitment to the health and well-being of the American people. Our success as an institution ultimately will be measured by our ability to demonstrate that the goals of the scientific community and the public are congruent. A foundation of mutual trust and confidence will hasten and strengthen our progress toward this shared vision.

To The Public:

The NIH shares with the American people an abiding commitment to the improvement of human health and enhancement of the quality of life. We exist to serve the people and, indeed, addressing their health needs is the essence and foundation of our mission. Our effectiveness depends on the ability to communicate new knowledge expeditiously so that enhancements are generated rapidly in the practices and technologies of the health sciences. We must not overpromise, but the goals we set will be of the highest order. We will respond to our fullest potential and mobilize the sum of our human and material resources toward conquering disease and disability. We will be sensitive and responsive to the people we serve, and work assiduously to hold and strengthen their trust and confidence.

To Science:

The NIH will search endlessly for scientific opportunities that will make innovative and valuable contributions to human health and to the length and quality of peoples' lives. New, expanded, and broad based opportunities in the life sciences must be pursued and applied with the determination to find solutions to human health problems and needs.

To Our Scientists and Employees:

NIH's success is the success of its scientists, who are assisted by dedicated professional and support colleagues. The life sciences must attract bright, creative, and highly committed individuals, giving them freedom to pursue their own unique insights, ideas, and perspectives. This can be accomplished only if we create an enterprise in both our intramural and extramural communities that ensures reasonable stability at an individual level and flexibility in the pursuit of knowledge; attends tirelessly to the renewal of talent, regardless of race, sex, creed, or physical disability; and, commits abidingly to the values of trust, integrity, intellectual generosity and openness, propriety in collaborative endeavors, and a spirited, yet measured, sense of competition.

To Our Nation's Youth:

The NIH must attract the youth of this Nation to science and to pursuing scientific careers. The new ideas and innovations of tomorrow depend upon the young minds of today. Our human resource base must be nurtured now so that the remarkable advances occurring across the frontiers of biology and biomedical sciences today will achieve the highest possible yield in future years.

To the Federal Stewards:

We must assure Congressional leaders and Executive Branch officials that we are investing public resources wisely and responsibly and that we are providing a strong return on the public investment. With equal measure, we must demonstrate our sensitivity to the social, legal, ethical, and economic concerns of our Federal leaders and remain responsive to their oversight responsibilities.

To Our Communities:

The country is our campus. The intramural and extramural programs of NIH are present in communities throughout the country, where our investment forms an important funding base, source of employment, academic enhancement and means for enrichment of the quality of regional medical care. NIH, as a full or partial employer of over 100,000 people, must recognize its responsibility as a citizen in local communities throughout the country.

To Our Extramural Research and Education Institutions:

The ties between NIH and the extramural community are long standing, reflecting the healthy interdependence that characterizes many of today's advances in science. The educational and research institutions comprising the scientific community form a vital part of the health research enterprise. In terms of physical infrastructure and intellectual base, they represent a national resource that is critical to the quest for scientific knowledge. NIH is committed to maintaining strong partnerships with these institutions and, in so doing, to nurturing the pool of scientific talent that makes possible exciting and promising breakthroughs in life sciences research.

To Growth of the Enterprise:

Biomedical and behavioral science is an endless frontier, and an enterprise that must grow to be sustained. As long as disease and disability continues to impair human life and health and our population continues to grow, become more diverse, and live longer, expansion of the enterprise becomes even more imperative. This growth must be driven by the scientific accomplishments of today and the commitment to more powerful and effective discoveries of tomorrow, rather than by routine budget mechanisms and accounting practices. Achieving the necessary level of growth will require a national commitment to NIH as a priority--a willingness to make health sciences an Executive Branch and Congressional strategic goal--as well as improved management of existing resources.

To The Nation's Economy:

Transferring knowledge efficiently and quickly constitutes a vital part of NIH's mission. The biotechnology, pharmaceutical, and medical device industries, which in large part stem from NIH supported research, have been and will continue to be major forces in advancing the Nation's economic growth and productivity. Biomedical and behavioral research is an effective national strategy in addressing one of the Nation's most intractable social and economic problems--containing and reducing health care costs. The ultimate approach to spiralling health care costs must be to prevent and cure disease.

To Leadership in the Life Sciences and Medicine

Leadership in biology and medicine has been one of America's greatest contributions to our time. As the world's leading institution for the support and conduct of biomedical and behavioral research, the NIH plays a global leadership role in the health sciences. The discoveries of NIH-funded scientists have advanced the health status of Americans and populations around the world and spawned the development of new technologies and industries that are global in their range and impact. While contributing to the development of world class institutions in the United States, NIH's leadership and commitment to research have been emulated throughout the world. Our leadership has further served as a vehicle for national security and for positive linkages to established and emerging countries throughout the world. In the area of medical care costs, we also have a leadership role to play because basic and applied research reduces the burdens and costs of chronic illness. Our efforts in this area will contribute immeasurably to the social and economic well being of the country. We must continue to provide leadership in marshalling critical expertise and resources to confront the health problems that challenge this nation and all nations.

TRANS-NIH OBJECTIVES

OBJECTIVE 1 -- CRITICAL SCIENCE AND TECHNOLOGY

Assure that critical science and technology in basic biology impacting on human health and the national economy are advanced as priorities across the NIH.

Investment in critical science and technologies in basic biology will set the stage for future advancements that will improve human health, reduce health care costs, and bolster the nation's economic well-being. The suggested operational components of our discussion framework that relate to and advance this objective are:

- Molecular Medicine
- Biotechnology
- Vaccine Development
- Structural Biology

These areas transcend categorical institute missions but are central to each of them in that they contribute in substantial ways to the understanding of most diseases. Additionally, they offer longer term promise of containing and reducing health care costs. Now and even more so in the next five to 10 years, these areas offer substantial contributions to the enhancement of the nation's economic growth, productivity and competitiveness. They are "Investing in the Future."

OBJECTIVE 2 -- RESEARCH CAPACITY

Strengthen the capacity of the national biomedical and behavioral research enterprise to respond to current and emerging public health needs.

A strong research capacity ensures that the public health needs of today are addressed and that disease prevention and quality of life are advanced. The NIH framework for discussion highlights major cross-cutting areas that are of particular importance to all of NIH. It identifies diverse areas of research, including targeted activities, that are essential to NIH's health mission. The suggested components are:

- Basic Biology and the Environment
- Neuroscience and Behavior
- Childhood Health and Mortality
- Reproductive Biology and Development
- Prevention, Health Education, and Disease Control

- Population-Based Studies
- Chronic and Recurrent Illness and Rehabilitation
- Aging
- Health of Women, Minorities, and Underserved Populations

For the most part, the individual Institutes of the NIH reflect long-standing National priorities to respond to today's public health needs. Strong Institutes with their focus on specific diseases and human health are central to achieving this objective. These suggested areas have been identified by the Institutes as ones which they will promote and develop. Together they will have a direct impact on whether we can achieve the goals of the Department of Health and Human Services and the Public Health Service as outlined in its strategic plan, <u>Healthy People 2000</u>.

OBJECTIVE 3 -- INTELLECTUAL CAPITAL

<u>Provide for the renewal and growth of the intellectual capital base essential to the biomedical research enterprise. Ensuring fairness and equality of opportunity at NIH is central to efforts to enhance the human resource base of biomedical research</u>.

The framework proposes operational issues in concert with the Secretary's Program Direction #7 (increasing the number of well trained biomedical and behavioral scientists) that are critical to maintaining and enhancing the talent pool of science, including strengthening research training and career development and ensuring the recruitment and retention of underrepresented groups into science. The suggested issues include:

- Science Education and Human Resource Development
- Intramural Research -- Research Infrastructure
- Professional Standards of Scientific Research

The NIH supports more training and career development in science than any other federal agency. This objective is vital to the future of NIH and the biotechnology industry, and is also key to achieving the President's education goals, <u>America 2000</u>.

OBJECTIVE 4 -- STEWARDSHIP OF PUBLIC RESOURCES

<u>Secure the maximal return on the public investment in the enterprise</u>.

Maintaining a diversified basic and clinical research portfolio managed by the individual Institutes, with emphasis on investigator-initiated research and quality management systems, undergirds this objective and its suggested functional components. These suggested components are:

- Technology Transfer
- Cost Management
- Intramural Research -- Research Infrastructure

Stewardship of public funds entails supporting outstanding scientists who advance the enterprise through their meritorious research performed in the interest of the public. Also inherent to achieving this objective is ensuring innovative and responsible managers and quality management systems. Above all, stewardship demands integrity and fairness in the conduct of our business.

OBJECTIVE 5 -- PUBLIC TRUST

Continually earn the public's respect, trust, and confidence as we carry out our mission.

The rapid progress and growing complexity of science and the public's heightened expectations for the research enterprise necessitates closer attention to social, legal, and ethical issues inherent in biomedical and behavioral research; to professional standards of science; to efficient communication of facts to the public; to the impact of research on health care; and, to transferring technology and fostering collaborative endeavors in the public interest. The following suggested components advance this objective:

- Social, Legal, and Ethical Issues in Biomedical and Behavioral Research
- Professional Standards of Scientific Research
- Science Education and Human Resource Development
- Communications and Information Flow
- Impact of Research on the Nation's Economy: Health Care and Biotechnology
- Technology Transfer

While the NIH consistently has ranked among the top three most respected government agencies, this cannot be taken for granted. As a public enterprise that is of vital importance to every man, woman and child in this country, we must hold ourselves to the highest standard. Only by so doing will we continue to deserve the public's trust.

GENERAL PRINCIPLES FOR IMPLEMENTATION

The strategic planning process charts a course for those efforts that are critical to the success of the entire enterprise. Implementation spells the success or failure of the planning process. Long-term planning does not imply micromanagement or attempts to specify the details of research or to interrupt the ongoing activities and priorities of the individual institutes. At the same time, no strategic plan can predict the full range of issues that are of importance to future development. The scientific process is such that the path of scientific discovery cannot be defined in advance. The NIH institute-wide leadership believes, however, that success in pursuit of our mission can be catalyzed and accelerated by judicious planning. Promising areas of research opportunity and important issues of science policy can be identified and advanced in the interest of achieving NIH's long term goals. A strategic plan is not a static document or an event, but rather a dynamic process that will evolve over time and use.

The implementation phase of a strategic plan will be guided by the following principles:

1. **The Institutes, Centers and Divisions (ICDs) are the Agents for the Implementation Plan.**

NIH Institutes and Centers represent a diverse array of missions and initiatives. Each has a mandated mission, legislatively defined priorities, and their own long range plans that address science and health matters from a specific perspective. Accordingly, the ICDs are the agents for implementing the trans-NIH strategic plan. The ICDs will be responsible for implementing specific initiatives, virtually all of which are pertinent to their individual research missions.

2. **The NIH Corporate Role in the Implementation Plan.**

The Implementation Plan complements the ongoing planning mechanisms of the ICDs. Oversight of the strategic plan's implementation will be conducted at a broad level. In many cases, initiatives will foster collaboration among several ICDs. Reflecting the mutability and unpredictability of science, the elements of specific initiatives will inevitably change. Individual project decisions with associated dollar levels will be made by the ICDs. At the broader level, responsiveness to the objectives of the strategic plan will be reflected in the development of the annual budget.

3. **Science Programs will be the Focus of NIH Budget Presentations**.

In past years, the development and allocation of the NIH budget has generally centered on mechanisms rather than on cross-cutting science opportunities and disease impacts pursued in the interest of human health. For many, the mechanisms, i.e., the number of research project grants funded annually, have come to be the measure of NIH's success. Under the NIH strategic plan, in the development of future budgets, the emphasis will shift toward the scientific and programmatic focus with mechanisms, albeit important, considered a means to the achievement of scientific goals and programs.

4. **Commitment to Scientifically Meritorious Investigator-Initiated Research**.

In emphasizing scientific and programmatic goals, we reaffirm the principle of high-quality investigator initiated research as essential to discovery. Indeed, a hallmark of the NIH since the 1940s has been its primary reliance on the freedom of individuals to pursue their own diverse ideas. This strategy has been successful in encouraging creativity and maintaining scientific freedom while fostering high quality. Investigator-initiated research is at the heart of scientific inquiry in which discoveries arise in unexpected places, from improbable insights and through leaps of imagination. The implementation of an NIH strategic plan will continue to rely on this commitment and should generate innovative ways, such as the newly established Shannon Awards, to support investigator-initiated research.

5. **Balance/Diversity of the NIH Research Portfolio**.

The NIH strategic plan is not intended to be an all inclusive list of research opportunities in modern medicine. Rather it highlights major cross-cutting areas that are of particular importance that must be part of the existing research base in every ICD. The NIH strategic plan addresses the health needs of the present and the future simultaneously. For example, Molecular Medicine, along with its associated areas of Structural Biology, Biotechnology and Vaccines, uses critical technologies that will provide new insights into the nature of health and disease to serve as a foundation for treatments of tomorrow. Due to their far-reaching and long-range nature, opportunities in these areas are a special focus in implementing the NIH strategic plan. Investment in this basic research portfolio is critical to progress.

NIH also serves the health of the public today through a research capacity that engages a wide range of biomedical research on disease and allows NIH to be responsive to the immediate and sometimes changing needs of the times. For example, the Women's Health Initiative will have direct impact on the health of millions of women through its studies of the effect of hormone therapies, diet, behavior, and exercise. Major clinical trials provide advances in treatment of many diseases, and these trials will be continuing to explore optimal strategies for management of today's patients, including studies specifically targeted at underserved populations. Nutrition research will examine practical options that will impact heavily on disease prevention. As we look at the health of the entire person, the need for trans-NIH studies that cut across specific disease- or organ-oriented Institutes will increase.

6. **Adherence to the Principles of Cost Management**.

The implementation of the NIH strategic plan will be carried out in cooperation with the principles set forth in A Plan for Managing the Costs of Biomedical Research. The NIH strategic plan reflects the need for stability and predictability in the funding level for biomedical research and for the wise and effective management of research costs. Indeed, the concept of cost management is embedded in the strategic plan and is explicitly covered as one of the ten critical policy issues that bear on the ability of NIH to fulfill its mission. The cost management policy issue sets forth the second stage of NIH's effort to examine the costs of research and to develop appropriate and judicious means of managing such costs. Large scale program expansions are not likely, given current budget resources and implementation of the proposed draft science initiatives may require redirection in existing programs.

While the particulars of cost management efforts will undergo continual reexamination and evaluation, the NIH strategic plan will be implemented in accordance with the framework established as the first step in the financial management plan. Abiding by the principles of the cost management plan is essential to ensure prudent and efficient stewardship of all NIH programs.

QUESTIONS TO BE ADDRESSED BY PANELS DURING NATIONAL AND REGIONAL MEETINGS

To focus the extramural community's review of the framework discussion of its objectives and strategies, the NIH has proposed a series of questions to be addressed during the national symposium and four regional meetings. The questions are categorized according to the proposed trans-NIH objectives and the panels that would address them. They are not the only questions that should be asked, and panel members are encouraged to raise others.

<u>Panel 1 -- Critical Science and Technology</u>

1. Regardless of the budget levels, is it appropriate for areas designated as critical science and technology to grow at a differential rate from the rest of the NIH budget?

2. How do we identify differences in opportunities that could single out components within programs for special emphasis in resource allocation? What are the appropriate criteria to be applied?

3. Are there any omissions in the critical science and technology area? Likewise, are there components presently included that clearly do not belong in such a grouping?

4. What data will be useful to collect in order to assess whether the research supported by the NIH is appropriate?

<u>Panel 2 -- Research Capacity</u>

1. Regardless of budget levels, should different components of the NIH, including the different institute initiatives, show differential rates of growth?

2. How can we ensure the responsiveness of the system to the public health needs of the Nation?

3. Comment on the suggested trans-NIH components. Are there additions or deletions?

4. What data will be useful to collect in order to assess whether the research supported by the NIH is appropriate?

5. How can we better inform the public about disease prevention measures and behaviors?

<u>Panel 3 -- Intellectual Capital</u>

1. How can the NIH determine whether we are training enough basic and clinical investigators in the life sciences to

meet our needs for the future? Is the distribution correct? Are we meeting our emerging needs, e.g. in biotechnology?

2. How can the private sector help the U.S. Public Health Service in the role of promoting public science literacy?

3. How should we address recruitment and retention of talent to biomedical research? How do we deal with the aging of the investigator pool, and the decline in grant applications from young scientists?

<u>Panel 4 -- Stewardship of Public Resources</u>

1. Regardless of the level of funding, how do we balance research, training, and infrastructure needs?

2. Is the current balance of scientific programs and NIH mechanisms right? Do you affirm the suggested principles of maintaining a diversified portfolio and emphasizing quality management? Any general comments on peer review?

3. What is the role of the intramural program in the NIH portfolio and how can its contributions best be sustained?

4. Under the cost management program, the NIH strives to achieve stability and predictability. Given increases in the requested size of individual grants that exceed increases in the overall NIH budget, should we intercede, to a point, to prevent contraction of the overall grant portfolio? That is, what is the appropriate trade-off between growth in the size of grants and the size of the total portfolio? (This discussion should be placed in the context of the following current statistics: the average actual cost of an NIH grant exceeds $200,000; and, the average cost of an NSF grant in biology is approximately $90,000.)

<u>Panel 5 -- Public Trust</u>

1. How do we achieve the right balance between maintaining a vigorous scientific enterprise and focussing on the areas of greatest concern to the public?

2. What is the appropriate role of the NIH in dealing with broad social, legal, and ethical issues that touch on biomedical research? How should we engage these issues? Should we avoid them?

3. How do we build the image and goodwill of the NIH in the minds of the public?

4. What are NIH's responsibilities in transferring research results into commercial products? Does, and should,

technology transfer opportunities substantially alter scientific priorities or the environment of science? How should we encourage socially responsible technology transfer efforts among the NIH scientific community?

Mr. BOUCHER. (Presiding.) Thank you very much, Dr. Healy.

Dr. Massey, we welcome you this morning and we'll be glad to hear your testimony.

Dr. MASSEY. Thank you very much, Mr. Chairman. I'm pleased to be here, Mr. Packard and other members of the committee. I have submitted a written statement, so I'll be brief in these oral remarks.

I am also pleased to be accompanied by Dr. James Powell, who is a member of our National Science Board and Chief Executive Officer of the Franklin Institute. Dr. James Duderstadt, who is the Chairman of our Board, apologizes for not being here this morning. He's also president of the University of Michigan and he was occupied last evening.

[Laughter.]

I want to address the questions in the letter you sent. The need to set research priorities is not a new issue, but it is more important now than it has ever been. This is due to a number of factors, including new opportunities and challenges, growth in the research enterprise, severe fiscal constraints, and the need to invest tax dollars wisely. I believe we have made significant progress in developing and strengthening mechanisms to set priorities in research and in Government. But we can do a lot better.

Let me address first the setting of research priorities within research fields. As this subcommittee is aware, our research enterprise, like our society as a whole, is pluralistic and largely decentralized. At the grassroots levels, priorities are set by individual investigators and individuals in research fields, and the most fundamental priority setting mechanism at this level is merit review. Merit review relies on scientists, engineers and educators to identify excellence, new opportunities, and progress in research fields.

NSF's own merit review criteria include the following: first, the competence of the researcher; second, the intrinsic merit of the research; third, the likely utility of the research; and fourth, the benefits of the research for the Nation's research and educational base.

Merit review is only one means by which research communities directly affect the priorities at NSF. Research and educators also help establish priorities through the National Science Board, which is a presidentially-appointed board of a mix of administrators, scientists, people from the private second and academia.

We also have a number of advisory committees. We receive advice through professional societies, through the National Research Council, and from individuals on temporary assignment at the National Science Foundation from the research and education community.

I believe the research communities can play even stronger roles in setting priorities. Currently, there are few fields of research that attempt to develop explicit priorities. The astronomical community is one example, and you will hear from a representative of that community this morning. There should be more attempts at setting priorities within fields. A clearer sense of direction from the communities themselves would certainly improve priority setting by the Government. Let me now turn to the process by which the National Science Foundation establishes its own priorities.

Planning within NSF is a continuous, "bottom-up" process. From the wealth of ideas obtained from outside sources, NSF identifies its priorities. These priorities are based on scientific readiness, technical feasibility, response to national needs, affordability, and balance with existing programs.

Since coming to NSF, I have established a new Office of Planning and Assessment and have instituted a new long-range planning process. This new process will be driven primarily by ideas and themes in research and education. It will also be more responsive to national and international needs.

The principal theme of this effort is to improve communication between the people and institutions that generate new knowledge and those that disseminate and apply it. We are currently in the middle of the first year of this planning process, and it will be a continuing and ongoing activity.

NSF also helps prepare the President's annual budget request. It is important to note that the administration views research as a high priority among all other activities. Agency budget requests are reviewed by OMB and OSTP, working with the agencies to provide an important top-down look at the Federal research enterprise.

The Federal Coordinating Council for Science, Engineering and Technology, or FCCSET, provides another means for improving coordination among research agencies. For example, the FCCSET process has developed key interagency technology initiatives in areas such as advanced materials, biotechnology, and high-performance computing. The greatest strength of the FCCSET process is that the agencies participate on a voluntary and consensual basis. Agencies participate because they are truly committed to particular initiatives, not because they are forced to participate by outside and possibly inconsistent or inappropriate mandates. The voluntary nature of the FCCSET process also allows each agency to focus on its own areas of strength.

The Science Foundation is a full partner in setting the FCCSET agenda. For example, I chair the Committee on Physical, Mathematical and Engineering Sciences, and other top NSF officials play leading roles in the other interagency committees as well.

Before closing, I must note that priority setting at the Federal level is, of course, not exclusively an Executive Branch activity. Congress, after all, ultimately determines national priorities and resource allocations. Congress then, as you are doing, must closely examine its own processes for identifying and supporting research priorities.

Currently, there are 42 authorizing committees, budget committees and appropriation committees, in Congress with jurisdiction over the Federal R&D enterprise. Clearly, any unity or consistency of purpose in the President's budget can become lost in this environment. This is most obvious for the multiagency FCCSET initiatives. However, the same diffusion of responsibility among the various committees affects individual agency budgets and non-FCCSET activities as well.

There are a number of options that could be considered. For example, multiyear authorization bills have lent greater stability and visibility to major research initiatives. We could produce even greater results with multiyear appropriations bills. Committees could be reorganized to simplify jurisdiction over research. Other changes to the congressional budget process may help establish a clearer, more consistent means for identifying and supporting research priorities.

I am aware that such fundamental changes cannot occur overnight, nor do I pretend that such changes, even if they were made, would necessarily always result in the same set of priorities by the Executive Branch and Congress, not even for the NSF—happy though that thought might be. What I do believe, however, is that we must all approach the process of setting priorities in a more rational, coherent and well-developed manner, one that can reasonably determine the trade-offs among areas of research as well as between research and nonresearch activities. The task must be performed by all participants in the research and policy-making area—in the research communities, the mission agencies, the administration, and Congress.

Mr. Chairman, that concludes my testimony. Dr. Powell will now briefly describe the role of the National Science Board in the priority setting process. Thank you very much.

[The prepared statement of Walter Massey follows:]

Testimony of Dr. Walter Massey, Director

National Science Foundation

Before the Subcommittee on Science

Committee on Science, Space, and Technology

United States House of Representatives

April 7, 1992

Chairman Boucher, Mr. Packard, and members of the Subcommittee, thank you for the opportunity to testify on the importance of setting priorities for Federally-funded research and on the planning and priority-setting process at the National Science Foundation. I am pleased to be accompanied by Dr. James Powell, a member of the National Science Board and Chief Executive Officer for the Franklin Institute. Dr. Powell will focus on the role of the National Science Board, which was chartered by Congress to set priorities for the Foundation. I will address the other questions raised in your letter of invitation.

The need to set research priorities is not a new issue, but it is certainly as important as it has ever been, if not more so. To illustrate, a National Academy of Sciences committee, at the request of Congress, once studied the organization of science in the Federal government. The NAS committee reported that the individual agencies supporting research were too independent and needed closer coordination and collaboration. That report was issued in 1884, but similar concerns have been expressed today. Priority-setting and effective management of the Federal research establishment are essential activities, especially in light of the sizable growth in the academic research establishment in the 1980s, continuing fiscal constraints, growing demands on the Nation's scientific and technological base, and other factors.

I do believe that we have made considerable progress over the last few years to develop and strengthen mechanisms for setting research priorities. These mechanisms take into account the diversity, dynamism, and progressive nature of the research enterprise and remain responsive to national needs as well.

Setting Priorities Within Research Fields

As this Subcommittee is aware, ours is a pluralistic, largely decentralized society. Our research communities and institutions are certainly no exceptions to this condition. It is reasonable, then, to begin a discussion of priorities at the "grass roots" level, that of the individual researchers and research fields.

Priorities in any given field of research are driven by a number of factors, but among the most important of them is the drive for scientific and technical excellence and progress. It is work of the scientist and engineer to push back the bounds of the unknown to discover what opportunities lay ahead or to reveal solutions to existing challenges. Research is a constantly evolving, self-renewing activity. New fields of research are being continually created, while mature fields are being combined and refocused, or phased out in favor of new areas of inquiry. The dynamic process of research provides a unique educational and training environment for students as well.

These themes of progress, excellence, and change in research are reflected in the most fundamental of priority-setting mechanisms -- the merit review process. Merit review relies on the expertise of scientists, engineers, and educators, who are most capable of recognizing new opportunities and challenges. Merit review also weighs other factors of equal importance to maintaining the strength of this enterprise. In the case of the National Science Foundation, merit review criteria include: (i) the competence of the researcher submitting the proposal; (ii) the intrinsic merit of the research; (iii) the likely utility and relevance of the research; and (iv) the potential effects of the research on the infrastructure of science and engineering, including benefits to the quality, distribution, and effectiveness of the Nation's scientific and engineering research, education, and personnel base. Through the merit review process, therefore, we assure that our highest research priorities continue to include both advancing the general good of the research enterprise and taking advantage of the intellectual capital and other benefits it creates.

The merit review process is one means by which representatives of the scientific, engineering, and education communities directly affect the priorities of the National Science Foundation. These communities are also deeply involved in planning and priority-setting for NSF. Consistent with the need to maintain close and effective working relationships with outside communities, NSF relies on a largely "bottom-up" process for identifying new opportunities and developing program concepts. This process follows no particular calendar: it can be driven by a scientific or technical breakthrough; the availability of a new technology; national or international concerns; or simply a new idea.

The National Science Board provides the framework by which the fruits of this process are incorporated into the Foundation's long-range plans, for the Board is statutorily responsible for setting the priorities and policies of the Foundation, within the context of policies set forth by the President and Congress. The Board is also the single most visible and influential source of outside guidance for NSF. The Board's membership, nominated by the President and confirmed by the Senate, represents the full range of scientific and technical disciplines supported by NSF.

NSF also uses other means to maintain an ongoing dialogue with the research and educational communities. NSF relies extensively on advisory committees for information and direction. The Foundation has over seventy chartered advisory committees and panels, comprised of nearly 900 scientists, engineers, and educators. Most of these committees represent particular disciplines or fields of science; a smaller number reflect the interdisciplinary nature of the programs they advise. Membership on advisory committees is balanced by discipline, institution, geography, and demography. Furthermore, one-third of the membership of the committees rotates annually, so there is a constant influx of new people and ideas into NSF.

Advisory committees generally meet twice a year; task groups and sub-panels meet more frequently. All committees are actively involved in the development of program priorities and plans, as well as assessments of current program activities. The Director and other senior staff routinely meet with advisory committees for face-to-face discussions.

Professional societies represent another major source of outside guidance in NSF's priority-setting process. Societies sponsor workshops, symposia, and other occasions for scientific communication, while representatives from professional societies often take part in meetings of NSF advisory committees. NSF's ongoing contacts, both formal and informal, with professional groups and associations, academic presidents and deans, industrial and Federal laboratory directors, and private foundations help ensure that concerns, advice, ideas, and information from all sectors of the research and education communities continue to be heard.

The composition of NSF itself is designed to maximize input from the research and education communities. About one-third of the scientific and technical staff at NSF is comprised of Visiting Scientists, Engineers, and Educators (VSEE), who serve in temporary assignments of one to three years duration. They bring to NSF direct knowledge of the priorities, opportunities, and developments in their fields, knowledge that they put to work by directing their own programs and through formal and informal communication inside and outside the Foundation.

NSF also benefits from advice from the National Research Council or one of its constituent academies to provide advice on programmatic, scientific, and policy issues. These reports often play key roles in priority decisions and program development.

Through these and other mechanisms, the research communities can identify and communicate to the Foundation their priorities in and across fields. I believe, however, that the research communities need to play stronger roles in setting priorities. Currently there are few fields of science which attempt to develop explicit priorities. The astronomical community is one example; there should be many more. A clearer sense of direction from the communities themselves would certainly improve priority-setting within and across the federal agencies.

Planning and Priority-Setting Within NSF

Planning within NSF is a continuous process, based largely on information and recommendations obtained through the extensive "bottom-up" process described above. This process produces a wealth of new and compelling ideas. NSF's explicit planning exercise selects those ideas that are to be emphasized in a particular program, initiative, or budget. These priority decisions are shaped by many considerations, including: (i) scientific readiness; (ii) technical feasibility; (iii) response to national needs; (iv) affordability; and (v) balance with existing programs. In particular, the development of our human resources, especially the attraction of more women, minorities, and disabled individuals to mathematics, science, and engineering, is an important component of all of our planning.

Since coming to NSF, I have created a new Office of Planning and Assessment and instituted a new long-range planning process, one that will more fully take into account new opportunities in science, technology, and education and that will be more responsive to changing national and international needs. Organizationally, I have separated substantive science and engineering planning functions from the operational budget preparation functions. The new planning process will be driven primarily by ideas and themes in research and education. Each NSF directorate will first prepare its own long-range plan in considerable detail, concentrating on scientific and engineering opportunities foreseen over the next several years but without reference to budgetary detail. Based on these individual plans, we will develop an agency-wide long-range plan that puts scientific, engineering, and educational issues first, with an overlay of agency-wide objectives and implementation procedures.

The principal theme of our planning effort is to improve communication between the people and institutions that generate new knowledge and those that disseminate and apply it. NSF research programs will be more closely linked to educational institutions and industries. We are currently in the middle of the first year of this new planning process, so it is too soon to evaluate its outcome. We expect to have our FY 1994-1998 plan ready for the NSB's review this summer.

The priorities identified through this long-range planning process will be reflected in our budget requests for FY 1994 and beyond. This Subcommittee is familiar with NSF's process for preparing its budget, so I will not dwell on its details. I will merely point out that throughout this process NSF's management relies on all of the mechanisms for identifying research priorities described earlier -- the NSB, advisory committees, societies, the five-year plan, etc. -- as well as other mechanisms for identifying and responding to national priorities.

Setting Priorities Across Agencies

In addition to preparing its own budget, NSF also participates in government-wide preparation of the President's annual budget request. Both the Bush and Reagan Administrations have made research and education very high priorities. This simple fact should not be lost in our discussion of setting priorities within research -- the Administration views research itself as a high priority among other Federal activities. For example, the original decision to seek a rapid doubling of the NSF budget and the recommitment by the Bush Administration emerged from top-level deliberations involving NSF, OMB, the Office of Science and Technology Policy (OSTP), and the Cabinet. The Administration has also proposed a number of other initiatives in key areas of science and technology, which are summarized in the President's budget. Congress, and this Committee in particular, have often strongly supported these proposals.

The task of setting priorities among diverse agencies and competing needs and missions is formidable indeed. Various fields of research are supported by a number of mission agencies, of which NSF is only one. Overlap among agencies is common and is in large part due to the nature of research itself; different agency missions often require support from the same scientific fields. Setting priorities among these various missions remains an important need.

The R&D budget proposals submitted by NSF and the mission agencies are reviewed by OMB. In addition, OSTP provides scientific and technical expertise to OMB in preparing the President's budget request. Both OMB and OSTP can provide a top-down look at the Federal research enterprise to ensure that it is consistent with the Administration's national priorities without being unnecessarily redundant. The working relationships among the agencies, OMB, and OSTP have been considerably strengthened in recent years.

In particular, a new dimension has recently been added to the traditional process of budget formulation. The Federal Coordinating Council for Science, Engineering, and Technology (FCCSET) has become increasingly active in developing interagency research and education initiatives, in addition to examining other non-budgetary issues of interest to the mission agencies. FCCSET itself is not a new entity, having been established through legislation and executive order in the mid-1970s. FCCSET, however, has been revitalized under this Administration, and with considerable success. The interagency coordination and cooperation made possible through FCCSET allows the government to maximize its investment in high-priority areas of research and to undertake research and educational activities that are beyond the scope or resources of a single agency.

I have testified earlier this year on NSF's participation in specific FCCSET initiatives, so today I will focus instead on NSF's involvement in the FCCSET process. FCCSET, as you are aware, is not a separate agency; rather, it is better described as a forum or process by which the mission agencies, OSTP, and OMB may collaborate to develop targeted, efficient, interagency research programs and to examine issues of common interest.

The FCCSET process is voluntary and consensual, reflecting the diversity of missions and needs of the participating agencies. Furthermore, the members of FCCSET and its committees are high-ranking agency officials, able to commit their agencies to FCCSET activities as appropriate. The agencies are highly selective in the commitments they make. This is, perhaps ironically, one of the greatest strengths of the FCCSET process. Each agency determines its own degree of participation in FCCSET activities. Thus, an agency's participation in a particular initiative stems from its own commitment and interest, not from outside and possibly inconsistent or inappropriate mandates. The voluntary nature of the FCCSET process also allows each agency to continue to do what it does best. In NSF's case, this means support for research and education in mathematics, science, and engineering.

NSF is a full partner in setting the FCCSET agenda. I am personally a strong supporter of the FCCSET concept and procedures, and I have encouraged my colleagues to play strong roles in various components of FCCSET. I chair the Committee on Physical, Mathematical and Engineering Sciences, which has developed the High Performance Computing and Communications (HPCC) initiative. Our Deputy Director, Dr. Fred Bernthal, chairs the Committee on Earth and Environmental Sciences, which has developed the U.S. Global Change Research Program (USGCRP). Dr. Luther Williams, our Assistant Director for Education and Human Resources, is the Vice Chair of the FCCSET Committee on Education and Human Resources, which has developed the mathematics and science education strategy, "By the Year 2000: First in the World." NSF officials have also played important roles in the Advanced Materials and Processing Program (AMPP) and the biotechnology initiative, and I expect that NSF will continue to contribute to upcoming initiatives, as appropriate.

As an adjunct to the traditional budget formulation process, the FCCSET process, I believe, will continue to provide an important mechanism for synchronizing priorities among agencies and, in the process, will foster more effective and beneficial research.

Priority-Setting Across Branches of Government

I have developed my testimony on research priorities by starting at the "grass roots" and working upwards to the national level. Plurality, as I mentioned earlier, is a hallmark of our Federal research and education enterprise. I would be remiss, therefore, if I did not mention the most significant manifestation of our pluralistic system -- the separation of the Executive and Congressional branches.

Priority-setting at the Federal level is not exclusively an Executive Branch activity, for Congress ultimately determines national priorities and resource allocations. As you are well aware, Congress is entrusted with the national taxing and spending powers, as well as the power to create, reform, or abolish agencies and departments. Furthermore, through hearings such as this one, Congress offers various constituencies, including research and education communities, a very important forum for providing national policy-makers with information and advice on priorities, challenges, opportunities, and options. And through its oversight function, Congress helps ensure that the agencies are performing their missions effectively and responsibly.

Congress, then, must closely examine its own processes for identifying and supporting research priorities. Congress has divided responsibility for reviewing the Federal R&D enterprise among two budget committees, twenty-two authorizing committees, and eighteen appropriations subcommittees in the House and Senate. These figures do not include those committees with minimal R&D responsibilities and certain non-legislative committees, such as the Joint Economic Committee, which have shown an interest in science and technology.

Clearly, whatever unity or consistency of purpose may be developed in the President's budget can become lost among competing congressional committee jurisdictions. This is most obvious for the FCCSET initiatives. The High Performance Computing and Communications initiative, for example, is subject to review by at least seven authorizing committees and ten appropriations subcommittees in the House and Senate. For the Advanced Materials and Processing Program, these figures are about eleven and fourteen, respectively. The same diffusion of decision-making responsibility and lack of effective coordination, however, affects agency budgets and non-FCCSET activities as well.

This Subcommittee is well-familiar with this situation, so I will not dwell on it at length. Suffice it to say that options do exist, and should be carefully explored. For example, multi-year authorization bills have helped lend some stability and visibility to major activities, such as the doubling of the NSF budget (P.L. 100-570) and the High Performance Computing and Communications program (P.L. 102-194). Even greater results may be obtainable through multi-year appropriations bills. Reorganization of authorization committees and appropriations subcommittees may also facilitate the task of identifying and supporting priorities for Federal research in a clearer and more consistent manner. Other reforms to the congressional budget process may also be needed to improve this task.

I do not pretend that these are the only options, or that such fundamental changes can occur overnight. Nor do I pretend that such reforms would necessarily result in support for the same priorities identified by the Executive Branch and Congress -- not even for NSF, happy though that thought may be. What I do believe, however, is that we must all approach the process of setting priorities in a more rational, coherent, and well-developed manner, one that can reasonably determine trade-offs between areas of research as well as between research and non-research activities. And this function must be performed by all participants in the research and policy-making enterprises -- from individual investigators and research teams to institutions, societies, agencies, the Administration, and Congress.

Mr. Chairman, that concludes my testimony. I would now like to turn to Dr. Powell, who will describe the role of the National Science Board in the priority-setting process. After his testimony, we will be happy to answer any questions that you and your colleagues may have.

Mr. BOUCHER. Thank you, Dr. Massey.

Dr. Powell, we welcome you this morning.

Dr. POWELL. Thank you very much, Mr. Chairman, and members of the subcommittee. I'm very grateful for the opportunity to come and explain the role of the National Science Board in helping in the process of setting priorities in research.

The need to set priorities within the Federal scientific and technological enterprise is obviously well-known to this subcommittee and is the basis of this hearing and your current efforts, so I only want to highlight two general issues. First, to underscore that the task of setting priorities, as Director Massey said, has become increasingly important, and this is due to a number of factors: increased demands on our science and technology base, the continuing fiscal constraints that Mr. Boehlert spoke about, increasing research costs, and finally, the need to assure the public that their tax dollars will be invested in a wise and rational manner.

My second point is that the important task of setting priorities must take into consideration the uniqueness and the complexity of conducting research. Any priority-setting process that we might envision must weigh several factors: excellence of the research, flexibility, direction, and other complex and competing features of the research enterprise.

For example, while we can and should direct our research efforts to address strategic national needs and research opportunities, we must simultaneously temper this sense of direction with some balance and some flexibility. Scientists know that the Nation and other nations of the world have clearly benefited in unexpected ways from long-term investments in research. These unexpected benefits stem from the unpredictable dynamic nature of science and technical progress.

The Government relies extensively on outside boards and panels of scientists, engineers and educators drawn from academia, industry, and other sources to obtain fresh perspectives, ideas, and information for setting priorities—and Director Massey explained how those apply to the Foundation.

The National Science Board is among perhaps the most prominent of these sources of outside guidance. The Board's membership represents the range of scientific and technical disciplines supported by NSF. As far as I'm aware, the Board is a unique Washington institution, in that it is the only board of outside experts that is statutorily charged with establishing the policies and approving the programs and activities of a major science agency.

The National Science Board executes this statutory mandate in three primary ways. First, it is closely involved in preparing, approving, and overseeing, the Foundation's annual budget request. In recent years, for example, the Board has assigned high priority to education and human resource development. There seems to be a consensus among most people thinking about science policy that education and human resources is where a good deal of our effort ought to be put.

Second, the Board helps develop NSF's long-range plans. The Board regularly studies and reports on issues of significant interest to the Foundation. These reports have motivated and directed NSF activities in many diverse areas. These include, for example, precollege and undergraduate education, graduate traineeships, advanced scientific computing, engineering research, biodiversity, polar research, international scientific cooperation—and there are still others that I haven't named.

The Board has also emphasized the importance of fostering closer ties between NSF and industry, and has encouraged NSF to focus more strongly on its contributions to economic productivity, competitiveness, and the quality of life. As a result of the Board's efforts, I believe that the Foundation is a very different organization than it was only a decade ago, and NSF will continue to evolve as needs and opportunities require.

The third primary means by which the Board guides the priorities and activities of the Foundation is through the review and approval of major programs and awards, which we define as exceeding a total of $6 million, or $1.5 million in a single year. If that's the case, then the Board has to approve those by a formal vote . The Board also evaluates ongoing NSF programs and reviews a wide range of other activities.

The National Science Board's responsibilities are not limited to overseeing NSF, however. The Board is also responsible for recommending and encouraging national research policies as well. This activity has ebbed and flowed over the years, depending on the particular issues at hand, the concerns and receptivity of the scientific and policy-making communities, and other factors. The Board has, however, explored a number of national issues. For example, it offered recommendations on financing academic research facilities, maintaining the openness of scientific communication—a panel that I happen to be on, foreign involvement in U.S. universities, and several other areas. Furthermore, the biennial report on Science and Engineering Indicators is a very important source of information on science, technology and education in the Nation that I believe is increasingly useful and referred to, and that, of course, is a publication of the Board.

So, in sum, the task of setting priorities for our science and technology enterprise is both very important and very complex. Solicitation of outside advice and review is an essential mechanism by which Federal agencies, including NSF, establish effective priorities. Through various means, the Science Board will continue to fulfill its statutory mission of providing oversight and direction to the Foundation, as well as promoting national policies for research and education.

Thank you again, Mr. Chairman, for this opportunity to testify, and I would be happy to answer any questions you might have.

[The prepared statement of James Powell follows:]

Testimony of Dr. James Powell, Member

National Science Board

Before the Subcommittee on Science

Committee on Science, Space, and Technolcgy

April 7, 1992

Mr. Chairman, Mr. Packard, and members of the Subcommittee on Science, thank you for inviting me to testify on setting priorities in research and on the role of the National Science Board in this endeavor. The Board, as you are aware, is charged by statute with setting priorities for the National Science Foundation.

The need to set priorities within the Federal scientific and technological enterprise is well-known to this Subcommittee, so I will highlight only two general issues in this regard. First, the task of setting priorities has become increasingly important, due to a number of factors. These factors include the growth of the research establishment; increasing demands on the Nation's science and technology base, brought about by new opportunities and changing national needs; the continuing fiscal constraints under which the government must function; the rising costs of doing research; and finally, the need to assure the public that their tax dollars will be invested in a wise and rational manner.

My second point is that the important task of setting priorities must also take into consideration the uniqueness and complexity of the scientific and technological activities for which these priorities are being set. For example, while we can and should direct our research efforts to address strategic national needs and research opportunities, we must simultaneously temper this sense of direction with balance and flexibility. The Nation benefits in unexpected ways from long-term investments in research. Research investments by NSF are simultaneously investments in the education and training of future scientists and engineers. These benefits stem from the unpredictable, dynamic nature of scientific and technical progress. Such progress, finally, demands excellence from our researchers and institutions and responsiveness to the research opportunities and challenges that they identify in their proposals. It is only through such excellence and responsiveness that we can truly discover and understand the world around us, and thus take full advantage of the opportunities created by science and technology.

These basics -- excellence, flexibility, and direction -- are only a few of the complex and competing features of the research enterprise that must be reflected in any priority-setting process. In the Federal government, these features are given expression through the extensive use of outside boards and panels of scientists, engineers, and educators, drawn from academia, industry, and other sources. These experts link policy-makers to the scientific and technical communities inside and outside government, providing fresh and current perspectives, ideas, and information. Furthermore, these experts, insulated from day-to-day political pressures, help recognize new opportunities; identify long-term needs and trends; and help establish or redirect programs and policies to serve such needs. Finally, such outside experts, representing contributors to and users of the Nation's scientific and technological base, are intimately aware of the importance of maintaining the excellence and vigor of this enterprise.

The National Science Board is among the most prominent sources of outside guidance for the federal research enterprise. The Board's membership, drawn from the ranks of preeminent scientists, engineers, and educators in academia and industry, represents the range of scientific and technical disciplines supported by NSF. The 25 members of the NSB are appointed by the President with the advice and consent of the Senate. The Board is organized into standing committees, which continuously examine current and proposed NSF activities and policy issues. Furthermore, the Board establishes special task forces, ad hoc committees, and commissions to deal with matters of particular complexity or urgency. The Board also frequently invites other individuals to take part in its deliberations.

The Board is a truly unique institution. It is, to my knowledge, the only board of outside experts that is statutorily charged with establishing the policies and approving the programs and activities of a major federal agency. The Board was originally envisioned by Dr. Vannevar Bush in his seminal work, Science: The Endless Frontier, the same document that advocated creation of the NSF itself. Both the Board and the Foundation were created by the "National Science Foundation Act of 1950," which explicitly charged the Board with "establish(ing) the policies of the Foundation within the framework of applicable policies as set forth by the President and the Congress" (42 U.S.C. § 1863(a)).

The National Science Board executes its statutory mandate in three primary ways. First, the Board is closely involved in preparing and approving the Foundation's annual budget request. The Board's participation in planning and budget formulation follows a regular cycle. At the June meeting of the NSB, NSF staff present recommendations and findings to the Board, which in turn indicates which of the proposed activities or issues it finds most compelling. The Board provides general guidance to the staff as to the size and scope of the budget proposal that should be prepared. In recent years, for example, the Board has assigned high priority to education and human resource development. From mid-June to August, NSF prepare the budget request, which the Board then reviews and eventually approves during its August meeting. The budget request is submitted to the Office of Management and Budget by September 1.

The NSF's budget requests are prepared and reviewed in the context of the Foundation's long-range plan. This is the second means by which the Board establishes the priorities and policies of the Foundation. As part of the Foundation's planning function, the Board regularly undertakes studies and reports on issues of significant interest to the Foundation. NSB reports have given major impetus to NSF activities in areas as diverse as precollege, graduate, and undergraduate education, advanced scientific computing, engineering research, centers and individual investigator awards, international scientific cooperation, polar research, and biological diversity. The Board has emphasized the importance of fostering closer ties between NSF and industry, and has encouraged a stronger focus on the NSF role and contributions to economic performance and the quality of life. NSF will continue to evolve as needs and opportunities require. By identifying and recommending areas for change, the National Science Board helps the Foundation maintain its direction, flexibility, and commitment to excellence.

The third primary means by which the National Science Board guides the priorities and activities of the Foundation is through the review and approval of major programs and awards. The Board approves all grants and contracts by the Foundation that exceed a total of $6 million or $1.5 millon in a single year. In its most recent meeting, for example, the Board approved four multi-year awards totalling $33.3 million. Three of these awards will support education and human resource activities; the fourth will provide research support for the International Institute for Applied Systems Analysis (IIASA). The Board also evaluates ongoing NSF programs, such as options for the future of the NSF-supported advanced supercomputer centers and the first class of engineering research centers. Finally, the Board reviews a wide range of other NSF activities, including its participation in the Federal Coordinating Council for Science, Engineering, and Technology (FCCSET), findings by NSF's Office of Inspector General, and other NSF activities and policies.

The National Science Board's responsibilities are not limited to overseeing the policies and operations of the National Science Foundation. The organic legislation that established the NSB charged both the Board and the Foundation's director with the task of "recommend(ing) and encourag(ing) the pursuit of national policies for the promotion of research and education in science and engineering" (42 U.S.C. § 1862(d)). This activity has ebbed and flowed over the years, depending on the particular issues at hand, the concerns and receptivity of the scientific and policy-making communities, and other factors. The Board has, however, explored a number of issues that are not limited solely to NSF. For example, the Board has provided reviews and recommendations on financing academic research facilities; maintaining openness of scientific communication; science and technology in the context of the closer integration of Europe; and foreign involvement in U.S. universities. Furthermore, the biennial report on "Science and Engineering Indicators," an important source of information on science, technology, and education in the Nation, is a publication of the National Science Board.

In sum, the task of setting priorities for our research and education enterprise is both important and complex. It is important due to pressing demands, growing opportunities, and limited resources, yet complex due to the need to balance the competing needs for excellence, flexibility, and attention to national needs. Solicitation of outside advice and review is an essential mechanism by which Federal agencies, including the National Science Foundation, establish effective priorities. Through various means, the National Science Board will continue to fulfill its statutory mission of providing oversight and direction for the Foundation, as well as promoting national policies for research and education.

Thank you again for this opportunity to testify, and I would be happy to answer any questions the Subcommittee may have.

Mr. BOUCHER. Thank you very much, Dr. Powell, and on behalf of the subcommittee, I would like to extend our appreciation to all of our witnesses who have appeared this morning.

For the initial round of questions, the Chair recognizes the Chairman of the full Committee, the gentleman from California, Mr. Brown.

Mr. BROWN. Thank you very much, Mr. Chairman. Let me add my appreciation to that of Chairman Boucher for your being here this morning.

The question of adequate funding for basic research has been, of course, very frequently discussed in the scientific community, and I'm recalling Dr. Lederman's study last year and various other indications of unhappiness in the scientific community.

Let me ask you this question. Is there a level of funding for scientific research, basic research, as conducted at NIH and NSF that you think would satisfy the scientific community? All of you.

Dr. MASSEY. I yield to the agency with the largest budget.

[Laughter.]

Dr. HEALY. I don't think that probably there is a level that would satisfy the scientific community, but I think the more important question is, is there a level that would satisfy the opportunities and the needs of the public. I think, framing the question that way, that we probably could come up with a level that would be appropriate. And it may not be as high as what the scientific community would want, but I think it would go a distance towards making them a little more satisfied, if that's the word.

Dr. MASSEY. I would agree with that. Since I've been at the agency, I've asked the same question of ourselves; that is, what should our budget be really in not-an-ideal world but in a realistically optimal world. I think one could arrive at a budget and it would not be of a shocking size compared to what we have now, and would allow us to address some of the very exciting opportunities that we are now not able to address, and to support some of the exciting individuals and work that is going unsupported. I think we could, together, come up with a budget that would reflect, as Bernadine said, what the public needs are and a realistic budget in terms of what the public could support.

Mr. BROWN. Dr. Powell?

Dr. POWELL. Well, I would agree with my two colleagues. I think it's in the nature of science that scientists have more ideas than they have time to follow or money to follow. One research project always generates more questions than answers, it seems, so it's in the nature of things that there's an exponential curve of rising expectations and needs.

But I would agree with Dr. Massey, that I think, working together, one could come out with an optimum level that would not satisfy all scientists by any means but would be satisfactory from the national point of view. I think this line of thinking is probably what led a few years ago to the notion that the NSF's budget ought to double over a period of time—something that was broadly supported by a lot of people.

Mr. BROWN. Well, you have a situation, Dr. Massey, where your agency is very highly thought of in both the Congress and the administration, the Executive Office. The President has committed to doubling your budget in a fixed period of time. Dr. Healy, your agency has had the longest, most consistent growth rate of any agency, in terms of supporting science, over the last 30 years. You indicate that you think you could calculate a level which you could satisfy the public need, but the public need for knowledge about health, for example, or knowledge about the universe, is essentially insatiable.

What criteria do you apply, both of you having rapidly-rising budgets, to determine what's the correct rate of growth or the rate at which you would begin to plateau off or what?

Dr. HEALY. Mr. Chairman, with regard to NIH, I think we have a very focused mission, in that ultimately, all of our advancing of knowledge and all of our scientific exploration are to be directed toward addressing the health needs of the public. So, to that extent, our priority setting always has to factor in, first and foremost, what are the pressing public needs. I think the AIDS crisis was an excellent example of something where, whether we had the increase in our budget or not, we had an obligation to respond to the AIDS crisis. In fact, now ten percent of our budget is directed toward basic and clinical work, directed clinical work, exploring how we can eradicate and prevent this scourge.

Now, those kinds of responses do not always match with the kind of budgetary increases that come, because budgetary increases often are a function of your previous year's budget. Whatever it was last year, you see what kind of a percentage increase you can get. There has never been a mechanism where there is a crisis increase, where for a period of time you will pour in money, maybe a ten-year commitment, maybe a five-year commitment, and say this will be directed as add-on crisis money to deal with this problem, and let the rest of the base grow at some reasonable level. In the case of NIH, it has been single digit inflation for the most part—or single digit increases for the most part.

One of the things we're trying to do as we struggle with the strategic plan is to say how can we put some reason into differential growth rates across different programs and different scientific areas in the NIH budget, and I think there would be two factors that would relate to a differential growth rate formula. One would be a pressing public need, or health emergency, and the second is extraordinary opportunity.

Mr. BROWN. Well, I'm asking questions which I hope will be provocative, because I have not been able to come up with any good answers to these questions myself.

Is there an external criteria that you can use? For example, comparison with the rate of expenditures in other industrialized countries, something of that sort, that has any value in connection with trying to determine what is the appropriate level for funding basic research?

Dr. HEALY. Me?

Mr. BROWN. Yes. I would like both of you to answer.

Dr. HEALY. I think that there are a number of measures like that. We have been looking at a range of them. We've looked at the measure of investment versus other industrialized countries, and what we see is that our biggest economic competitors, Japan and

Germany, have had a proportionately greater rate of rise of their investment in R&D, as you well know.

The second thing we look at we call "intellectual capital", the number of scientists and engineers per capita, and we have led the world for the longest time. We have plateaued and started to decline, and again, not surprisingly, our two largest economic competitors, Japan and Germany, are in a steep rise. In fact, I think Japan has just crossed the line and has exceeded us on numbers of scientists and engineers on a per capita basis. I think that's a good measure of the intellectual capital base of this country, and a good indirect measure of ultimate future productivity, if you believe technology is linked to the productivity of this country and its future.

The third thing we look at, which is more a basic science measure, is market share in publications. Again, the market share of the United States across the sciences, including mathematics, including chemistry and physics, but especially in biology and medicine, has been eroding. If you look at U.S. publications as compared to other of our industrialized partners in the world, and if you look at citations, if you look at scientific meetings—I will give you one anecdote which again is the kind of outcome measures that we're trying to assemble to address your question, one anecdote which is something we're pursuing right now to get broader data on.

Some time ago I was president of the American Heart Association, and we have one of the largest scientific meetings every year on all aspects of cardiovascular medicine—stroke, heart disease, basic science, applied science. And I chaired that scientific session for several years back in the early mid-80s. That scientific session draws people from all over the world. It usually has 20,000 people in attendance; it's one of the largest meetings of its kind anywhere in the world.

At the time I was chairing the scientific sessions, we would get in the range of about 20,000 abstracts, which were scientific papers for presentation. Somewhere between nine and ten percent came from outside of the country. This past year, I have been informed by my colleagues with this meeting that 49 percent of the papers submitted for presentation came from outside of the United States. There was a dramatic decline in the number of papers coming from within the United States.

So those are market share measures that I think we ought to look at and I think we ought to worry about.

Mr. Brown. Would you respond briefly—and this will be my last question, Dr. Massey—particularly since your institution funds research across a number of disciplines, is there any external criteria you can apply as to the relative amounts that would be optimum, or in toto, since you fund a lot of the university research and all of the sciences?

Dr. Massey. I think there are—the one you mentioned, we certainly should look at how we invest in long-term research and compare it with other countries, and especially nondefense related research, where we certainly don't compare very well. We should also look at measures of how well we are doing in training and educating the next generation of people who not only participate in science and engineering research but those who will understand it and have to use it. Again, we can see that we're not doing very well in that regard. So there are a number of external measures such as that, I think, that are quite appropriate.

We can match those against our more internal criteria, since our mission—as you say, NSF is not mission agency—our purpose is to broadly support the health and vitality of the Nation's research and education enterprise. We can see how well we're doing in that regard, in terms of the ideas that go unsupported, and the scientist's and engineer's research that we can't support. We can look at the grants coming in, and just through our advisory process get a measure of how much we could spend and yet not have to degrade the quality of our research.

So I think by judicious comparison of what measure of spending we would come up with by looking at our internal criteria, and measure that against the external factors, if there was some consistency in that regard, that might give us confidence that we are on the track to finding at least a reasonable level of investment that the public would support.

Mr. BROWN. May I just conclude by making a brief observation, subject to verification, of course. But my observation is that if there is any field of science which is enjoying a healthy growth rate, the number of students attracted to that field and research in that field will increase at a slightly faster rate than the rate at which its growing. That's a hypothesis which helps explain the dissatisfaction with the amount of money available, because the number of researchers always goes faster than the amount of money.

Thank you, Mr. Chairman.

Mr. BOUCHER. Thank you very much, Mr. Brown.

The gentleman from California, Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman. I appreciate your testimony, all three of you.

The National Science Board has been setting priorities for the National Science Foundation. What criteria are used in determining what research activities will be initiated? And I would be particularly interested in whether you have different criteria in evaluating big science versus small science and big science competing with small science projects.

Dr. POWELL. I could take a crack at that first, Mr. Packard.

The Board uses in a general way the four criteria that the Director mentioned, we look at projects, as I mentioned to you, above a certain dollar level, and $6 million is the level in which we get involved. That's hardly big science nowadays. The largest project that has come before the Board since I've been there is the LIGO project, and the Board examined that extremely rigorously over a series of meetings employing the criteria that the Director indicated. Even that project, though, I think was around $100 million at the time we did that, which is well below some of the projects that Mr. Boehlert and others have mentioned. So I believe that the Board gives those kinds of projects a good going over. But I think if you just look within the NSF, you're not really talking about big science projects.

Dr. MASSEY. In terms of balance between projects and individuals, the Board looks at the entire budget at the end of the year and tries to see that we have a balance, assuming everything in that has already gone through our merit review process. So that's done.

We are giving the highest priority now to maintaining the support of people, individuals or small groups of investigators—and the Board has agreed with that priority—and the second is to provide the scientists and engineers with the instruments to conduct their research. Now, some of those instruments sometimes are large, such as a telescope, but for us, that's very rare by and large.

And the third priority, broadly speaking, is to provide the facilities at the institutions where we support most of our research academic institutions. So within those three broad criteria, the Board then looks for balance across the spectrum of types of projects.

Mr. PACKARD. Dr. Healy, does the National Institutes of Health have a similar policymaking board, and how do you establish your priorities between the 20 institutes that you have at the NIH, and do those priorities drive the budget, or does the budget tend to drive the priorities?

Dr. HEALY. Well, we have a more complicated advisory mechanism, because each of our institutes has their advisory council, and under each of the advisory councils they have several additional advisory groups that feed into the advisory council. And then, at a level below that, we have a very elaborate peer review which looks at the grants that come in to NIH. I think you really have through that a three-tier level at each institute of micro to macro priority setting.

In addition, you have an advisory council for all of NIH, which is the Advisory Committee to the Director, which reviews priorities and discusses trans-NIH issues, and within NIH we have a body which we call the ICD meeting and an executive council which comprises the leadership of those 20 institutes, centers and divisions, and they participate in integrating setting priorities that we call trans-NIH priorities. So we have the priorities of the institutes interlocking with the trans-NIH priorities, and hopefully reinforcing each other.

There is also no doubt that our budget very much is the statement of our priorities, and the appropriations process for NIH is rather elaborate. As you know, we have two weeks of hearings that we just completed, and there both the NIH Director and each of the institute directors are asked about their scientific priorities in their testimony and how the allocation of their budgetary resources is made.

If I would fault our priority system in one way, it is that we, as a community—and it's not the entire global community of NIH—have tended to try and explain ourselves solely in terms of one priority, which is called an R-O1. I have often said that that sounds like a secret password to a fraternity. What does R-O1 mean? It certainly doesn't mean anything to the public. Well, R-O1 is the grant system of investigator-initiated research. And although that is an important means whereby we do our research, I don't think that should be held up as NIH's priority. NIH's priority should be articulated clearly to the public in terms of scientific programs, in terms of opportunities, and in terms of responding to the health needs of the people.

Mr. PACKARD. Thank you.

One of the concerns that members of this committee—certainly I have that concern, and I think the public, the American public, as they see their dollars used in research areas—is the fact that we often, in setting our priorities, fund the projects and start the projects, but we do not see the completion of those projects in some instances. That's not just true in our science research. That's true in virtually all of our procurement areas in Government, military, certainly space, and other areas. We find where we spend millions and sometimes billions of dollars moving a project along, only to find that after getting half-way through or part way through, it either becomes a lower priority or we, because of budget constraints, we discontinue it. That's a waste of significant moneys and certainly is an inefficient way of establishing and following through with our priorities.

How can we avoid that and what are we doing to avoid long-range planning that would facilitate us carrying our projects through to completion? Dr. Massey particularly.

Dr. MASSEY. On the planning side, as I said, I think we do very well. You know, we, at least, submit our budgets for our multiyear projects, such as our telescopes that we have in this year's budget, or the Laser Interferometer Gravitational Observatory. I think where the system does not work as well is the annual appropriation process, of course, when you have a multiyear project, and as you say, circumstances change, priorities change, and quite often then that leads to a lack of ongoing commitment to the project.

As you know, in many European countries funding for large projects is very different, in that the funding is committed for the total project at the time the project is approved and thereby the funds are guaranteed for the completion of the project. But that's another governmental system, of course.

Mr. PACKARD. Would multiyear budgeting help, do you think?

Dr. MASSEY. I certainly think so, yes.

Mr. BOEHLERT. Would my colleague yield on that point?

Mr. PACKARD. Of course.

Mr. BOEHLERT. Let me ask your reaction following along this line of questioning. Once we launch a project, should we feel duty-bound to carry it through to its logical conclusion, if somewhere along the way we properly exercised oversight responsibility and determined that our priorities had changed? It's a very dynamic situation. Let's get something completely out of the area of this committee, the DIVAD tank. I happen to think—and I think a lot of people in the Congress think—we made a very wise decision in the previous administration to cancel that project after we had spent a billion, eight hundred million dollars. But the conclusion reached at that point was that the situation had changed so dramatically that it would be folly to continue to pump money into that particular endeavor. A better utilization of resources would be to redirect them into a new higher priority area.

So I understand my colleague's line of questioning, and I can agree with your idea on multiyear appropriations and have a longer range point of view. But I hope no one would suggest that we should make a commitment to go forward with an endeavor and then never more consider whether or not we should continue to go forward.

Dr. MASSEY. I would think you could have a balance, Mr. Boehlert, in the system where you made the commitment and fund it, with the full expectation that if nothing major changed, you would go ahead. But I would assume Congress would always have that authority to reinvestigate if things change.

I think I was addressing the circumstances where it's automatically revisited every year, on the assumption that something has changed, when it may not have.

Mr. PACKARD. I think my concern is not so much that we ought not—I'm not suggesting we ought not to readdress and reevaluate our priorities. I think I would be suggesting, however, that we make certain that we do a better job, or at least a very successful job of evaluating our projects on a long-term basis so that we do not, either for political reasons or for other reasons, get half-way through the project and realize that maybe it wasn't a good decision in the beginning. That's what I'm concerned about, is that we allow politics or we allow poor planning, poor long-term planning in the setting of priorities, and I think that's the very purpose of this hearing, is to make certain that we do improve that to the extent we're capable, so that we don't make bad judgment decisions early on and pay dearly for them half-way through. And then, obviously, if it's a bad decision at the beginning, we ought to have the courage to make changes in our priorities.

Dr. HEALY. Mr. Packard, I think that one has to look at basic science and applied science in a slightly different way. In terms of basic science, which we fund mainly through investigator-initiated work, we have de facto, multiyear funding. Most of our awards are made for periods, on the average, of four years, and 25 percent of that portfolio turns over every year, so most scientists have a commitment that goes out to four years. NIH has experimented with longer grants, grants as long as ten years, and, in fact, we're starting to phase them out. I think everybody believes that four to five years is about as far as we should go to get the kind of recurrent revisiting that you're speaking of.

Now, in that basic portfolio, which are about 20,000 principal investigators, 22,000 more or less, we view science as an evolving story. I mean, it's a continuing process of discovery, so there is no discrete end to it. And even though you might have a changing pool of investigators, as some compete successfully to get in and others fall off the tree, so to speak, you still have a continuous process of discovery that does not have a discrete end.

Now, at the applied end of our work, which is 25 percent, 30 percent of NIH's budget, we do have discrete targets. One of them—for example, taxol. We have a very focused program to develop this cancer chemotherapeutic agent, which has very exciting new properties, a new kind of chemotherapy that is very promising for ovarian cancer and colon cancer. And this particular project is very focused towards getting other ways to either synthesize taxol or alternative sources from other than the Pacific yew tree.

We have ddI in the AIDS antiviral area. We've had very focused projects and we have a timetable, and we achieve an end point, and if we don't achieve it, we find out why and we will occasionally terminate.

The human genome project I think is our best example at NIH, of a quasi-large scale project, and we have very discrete goals and timetables and, indeed, we believe, if anything, we will achieve the mapping and sequencing of the human genome in less time, not a longer time, than we originally projected. Those kinds of very targeted research, which are not the bulk of our portfolio but are still a substantial amount of NIH resources, are tracked, are monitored, and have very specific goals and timetables that, for the most part, are achieved.

Mr. PACKARD. Mr. Chairman, I'm taking a little longer. May I ask one more question, however?

Mr. BOUCHER. Go right ahead.

Mr. PACKARD. I think we see more in military and space hardware where there are political decisions made, that often we have to reevaluate, and maybe not good decisions, pressures to put a project in somebody's district.

Is that often the case in our science project prioritizing, and if so, what can we do to insulate—Of all places we ought to make certain that our money is being spent in research prioritizing projects, it ought to be what the real priorities are and not what the political desires are. Is that a problem for your departments, your agencies, and if so, what can we do to insulate that system so that we do not find political decisions influencing unduly our prioritizing of science projects?

Dr. POWELL. I might say a word from the Science Board's point of view. I have been on the Board for six years. I've never seen a single instance where I felt what you described was the case. I believe that NSF's system of having intimate involvement with the scientific community, having program directors, and having an independent board, provides about as much insulation as you're going to be able to find.

Dr. MASSEY. I would just echo that. I think we are very fortunate to have had the support of Congress as a whole, the administration, and the community, for supporting our merit review process. So even when we go through a competition for what, to us, is a major facility to be constructed somewhere, such as the LIGO project—we just selected two sites around the country, a $200 million project. The process I thought worked extremely well, and although some states were disappointed, they accepted it because they respect the fact that we do carry out this process fairly. So, so far, we've been very good. We appreciate, in fact, the support of the system that allows us to operate.

Mr. PACKARD. We're pleased to hear that. Thank you very much.

Mr. BOUCHER. Thank you very much, Mr. Packard.

One of the suggestions that has been made to this subcommittee is that what we ultimately need is a somewhat more formal process at the executive branch level in making cross-cutting decisions with regard to various science projects, and establishing priorities in that manner.

A couple of recommendations have been made for how we might go about doing that. One is that we look to a potentially broader reach on the part of the National Science Board and let that unique board help set priorities across a broader range of projects than just the National Science Foundation. Another is that there be an institutionalization of what we now are witnessing through the FCCSET process at OSTP, and that perhaps that be formalized in some fashion.

What I would like to ask this panel, beginning with Dr. Powell, is to comment on those two proposals for a somewhat more formal structure and, if there's a broader range of proposals that we should examine for a somewhat more formal structure at the executive branch, to recommend those additional proposals to us.

So, Dr. Powell, let me start with you and ask you to comment, first of all, on the potential for the National Science Board, being the somewhat unique entity that it is, composed of 25 members representing virtually all disciplines within the scientific community, with its members appointed by the President and confirmed by the Senate, to have something of a broader role, to look beyond just the NSF and utilize the words of its charter which say "recommending and encouraging the pursuit of national policies for the promotion of research and education in science and engineering", in a way that would assist us both in the executive branch and in Congress in establishing a broad range of priorities for science projects.

Dr. POWELL. Mr. Boucher, I believe that the Board could do that. I think, in order for it to feel emboldened to do so, it would need the support and encouragement of committees like this one. The Board, as I mentioned in my testimony, has taken on certain issues—for instance, the issue of openness in scientific communication. It has not taken on other issues, such as whether we should have a supercollider or not. It has tended to stick with concerns that more directly involve NSF or that were pervasive and widespread and, therefore, weren't goring anyone else's ox.

What the Board has not done is taken a stand, let's say, on the human genome project, because it's felt that, perhaps while not outside its statutory purview, was outside good diplomacy and was being handled by another agency very well.

Mr. BOUCHER. Let me ask you this, Dr. Powell. I hear you saying that the Board has historically basically remained within the confines of programs of the National Science Foundation.

Dr. POWELL. By and large.

Mr. BOUCHER. By and large. Would it be appropriate, given the broader charter which already exists statutorily for that Board, for it to move beyond that mission and begin to help us in a somewhat more thorough way with national priority setting for science projects?

I guess another way of asking that question is, do you think that, given the broad membership of the Board, 25 people representing virtually all scientific disciplines, that you have within your membership the expertise to perform that function?

Dr. POWELL. I think the answer is yes, that we do. There is one potential conflict, however, and I think this is what has prevented the Board from reaching further in that direction in the past, and that is that we're in a competitive budgetary process and the Board, on the one hand, is responsible for overseeing the Foundation's budget, and then, if you ask us to also then take this larger role, how do we avoid being self-serving and appearing or, in fact, favoring the Foundation's budget over that of NIH or NASA or other Government agencies. I don't know that that's an intractable problem, but I think it is one that we have hit head on on several occasions, and when we have, we've tended to back away and stay more or less within the confines of NSF.

Your other question was about the FCCSET process, which I know about as a Board member, not as a person directly involved. I think that is a very encouraging development. Obviously, we've needed much more coordination than we've had in the past, and I think that Dr. Bromley and that committee have made a very good start on attempting to provide that. I'm sure Dr. Massey would agree that by involving yourself in that process, you give up a little bit of your flexibility and authority, but in return you gain some coherence and some consistency across the whole Federal Government, and personally, I think that's a good tradeoff. It could be that that mechanism could be expanded and strengthened.

Mr. BOUCHER. Would you recommend that, if we are to embark on the road of formalizing this structure within the executive branch for priority setting, that we try to do it by enhancing the function of the National Science Board and giving you a somewhat broader reach, or that we try to do it through the Office of Science and Technology Policy by expanding the FCCSET process and making that a somewhat more regular kind of approach with regard to all science funding? Which of those do you think is preferable?

Dr. POWELL. I think one might well want to do both and worry about how the Science Board could feed into the FCCSET process, which we now don't do in any direct way. The Board does have the statutory responsibility and I don't think we would want to—We may not have exercised it as fully as we could have, but we wouldn't want to give it up, either. So I think actually both things. I'm not trying to duck your question. I think both are worth serious consideration.

Mr. BOUCHER. Thank you very much.

Dr. Healy, you, I think, were a former Deputy Director of the Office of Science and Technology Policy. You also chaired the advisory committee that developed the OTA report with regard to Federal science funding that recommended that priorities be set in a somewhat more rigorous way than is the case today, so I think you're uniquely well-qualified to comment on this range of questions. We'd be happy to hear your response.

Dr. HEALY. Well, I guess I have to speak from two perspectives. As the Director of NIH, I am often saying that we are not the National Institutes of Life Science or the National Institutes of Biology, or the National Institutes of Science. We are the National Institutes of Health, and to that extent, we are more than a science agency. I think it is a little difficult to reconcile the added mission of the NIH and its role in what I view as domestic national security with being under an umbrella board which is primarily devoted to science in its broadest sense. That doesn't mean that a large part of what we do isn't science, but it is very mission-oriented science—perhaps more like defense science. It's just personal defense in terms of fighting off illness. So I think that it would be very hard to put NIH under the National Science Board.

It's not a territorial statement. In fact, I alluded in my comment that I think it was a very wise decision 50 years ago not to put NIH as part of NSF, which was Vannevar Bush's original recommendation, and it was actually a lot of jockeying back and forth and finally, by hook or by crook, the Public Health Service got NIH because, I think, of President Roosevelt's and President Truman's very strong belief that the health sciences somehow have a unique role in terms of public need and public interest. And, in fact, in the very elegant letter written by Roosevelt to Vannevar Bush, when he asked him to do his famous report, he singled out medical research and said can we do in the field of medicine what we've done in winning World War II.

So I think that I would be very cautious about going back on that formula. It's worked well for this country for 50 years and I think it's worked well to have the National Science Foundation, which has the unique mission of worrying about science and education and does not have to be as burdened by some of the very happy burdens that NIH has. I think it might be difficult to ask the National Science Board to take on that extra responsibility.

I think the second thing is, I personally, if you want me to be brutally blunt, don't think we have right now a mechanism within the executive branch of Government that adequately looks at all of the priorities in an open and robust and table-thumping way, which I think you need to do if you're going to have honest dialogue on what priorities should be—in anything, and particularly in science. There is not an opportunity to sit around the table with all of the agencies there, letting it hang out, saying let's look at SSC, let's look at the space station, let's look at NIH, let's look at NSF, let's look at training, let's look at the intellectual capital base of this country. We don't have the mechanism.

I think the FCCSET system has been revitalized under Dr. Bromley. It is certainly a much more effective system than it has been in the past. It has covered a wider range of issues. Some of its reports I think are spectacular, like the one on high-performance computing. I think others may not be as good, but I think it is more a coordinating mechanism. It is not a policy-setting mechanism and it really isn't a system where you have the high enough level official sitting down together and saying what, indeed, are our priorities; should we have some base closings in science. You know, those are the kinds of things that really raise blood pressure; those are the things that get at the heart of what this committee is struggling with. And I don't see that FCCSET can do it, I don't think the National Science Board can do it. I think we need to come up with something new.

Mr. BOUCHER. Okay. What's that?

[Laughter.]

Dr. HEALY. I knew you were going to ask me that.

Well, for one thing, I think that—Let me put back on my NIH hat again—in a very parochial sense, but I really believe this, and I would say it whether I was NIH Director or not—that NIH is not enough of a national priority, period. Now, I shouldn't say it in this room when I see all of this magnificent artwork, but in fact, I do not believe the public will for health sciences research is adequately reflected in our national priority setting. And that is not a partisan statement. That's across the board.

I think part of it is because we're fragmented. Isn't it sad that I've been NIH Director for a year and I've had endless testimonies, and this is the first time I've come before the Science Committee. Somehow NIH is not seen as the science priority that I think it should be, and in a dialogue within the scientific community within Government, we don't have that forum. I think we need to have a mechanism where all the science heads, where Dr. Massey and I, who occasionally get together maybe twice in a year, because we made a point of doing it. But there's no mechanism where Dr. Massey and I and the head of NASA and the head of DARPA and the head of EPA get together, the agency heads, the people who are in the policy hot seats, and discuss these kinds of issues. And I think something new has to be created, and it has to be something that has the confidence and the ear of the Congress and the confidence and ear of the President.

Mr. BOUCHER. Well, that's an intriguing proposal. So along that line, potentially we should create a Board of National Science Policy Directors that would be comprised of the heads of all the basic agencies that fund science in the United States, which in turn would talk about the various priorities within their agencies, look at the national interest, try and weigh and balance and come up with a set of recommendations.

Is that the basic recommendation you're making?

Dr. HEALY. You know, I'm suggesting that that's something that is lacking. I think that would have a little more effectiveness, at least in moving towards the kind of thing you're suggesting, than FCCSET, which tends to be people who are not necessarily in the policy position, who don't necessarily bring the clout of their agency, and I think that—I would suggest that maybe that body could help answer your question. So I'm not throwing out an answer to your question; I'm more throwing out a possible mechanism for getting there, and as I reflect on the questions that all of you raised, I though isn't this strange that, in a country that spends so much of its resources on science and technology, and is so science and technology dependent and driven, that we do not have a mechanism where the leaders of all the scientific agencies, defense and civilian, come together and talk, even talk.

Mr. BOUCHER. Well, that's a very intriguing recommendation and I appreciate it very much.

Dr. Massey, let me get your comment on that suggestion, and also on the question previously posed with regard to the potential of expanding the work of the National Science Board outside of its traditional functions.

Dr. MASSEY. That's why I'm in the middle, is that it?

[Laughter.]

From the question of the National Science Board, I agree with Dr. Healy. I think the Board, as presently constituted, and the way the system presently works, and given the range of responsibilities of an agency like NIH, I just don't see that as a practical solution. And also, given simply the workload of our Board in overseeing the Foundation, it would require a major restructuring.

The Board is not an advisory board. I just want to make it clear, unlike any other, the Act that created the Foundation says that the National Science Foundation shall consist of a National Science Board plus a director. So that is the National Science Foundation. They are legally required to do a number of things for the agency that I just don't think gives them the freedom, even if you wanted them to.

I have to take some issue with Bernadine on her assessment of FCCSET. I think it would be useful to have a body like that, but that's what FCCSET is. The full FCCSET committee consists of the Secretaries and the heads of the agencies. I think the difference is that the Secretary of HHS is a member of the full FCCSET and not the Director of NIH, whereas the Director of NSF is. I agree with her that that committee has not addressed these kinds of issues in that setting, but I think you don't need to create another mechanism is my point in order to have a body that brings together all of these people in order to address the issue.

Mr. BOUCHER. I think Dr. Healy's point is that, within the FCCSET process itself, the agency heads who are responsible for research funding aren't always represented, and that it's important that you get these individuals sitting around the same table.

I would take it that the way to draw a common ground between your two comments is to say that perhaps the FCCSET process might be upgraded to some extent to ensure that those people are, in fact, participants, and that the issue of priority setting be placed before them at least for the purpose of making recommendations.

OSTP within the administration is charged with the responsibility of coordinating science policy, and some kind of advisory board perhaps, under the direction of the head of OSTP, might be the right approach to take in terms of getting these kinds of cross-cutting functions put in place in a somewhat more regular basis.

Do the three of you generally agree with that? Is that a proper approach for us to embark on? Dr. Powell.

Dr. POWELL. Yes, I think so.

Mr. BOUCHER. Dr. Massey?

Dr. MASSEY. I think one can look at that. I don't think you need to upgrade FCCSET, though. I think, under Dr. Bromley's leadership, anything you're suggesting now can be done within the charter and framework of FCCSET.

Mr. BOUCHER. Why is it not being done?

Dr. MASSEY. I don't know.

Mr. BOUCHER. Dr. Healy? I'm sorry, Dr. Massey. I didn't mean to cut you off.

Dr. MASSEY. I don't think we can really address the setting of priorities, though, in the administration and not discuss OMB. I mean, really, in the end, the priorities that come over here, of course, are determined ultimately by OMB. So as you look for mechanisms to do that.

Mr. BOUCHER. Dr. Healy.

Dr. HEALY. It's been a curious phenomenon of post-World War II science that OSTP traditionally ignored NIH. In fact, when I went to OSTP when Dr. Keyworth was Science Advisor, it was unusual to have an Associate Director and a Deputy Deputy Director who was a life scientist. The emphasis of the office at that time tended to be more defense science.

Now that has changed, and Dr. Bromley has really moved it more towards civilian science. But there still, I do not believe, is a mechanism where the principals, the agency heads—and it may be someone as lowly as an NIH Director for the life sciences and someone as highly placed as the NASA Administrator or the head of NSF, but the fact is, when we talk about our daily operations and responsibilities for the science in this country, we are at the same level at that point, even though in terms of the bureaucracy our levels may not be as high.

So if you really want to get the job done, you've got to get the people together, even if they aren't matched in terms of lofty position but are matched in terms of responsibility, you've got to get them talking, and we don't have a mechanism where that occurs. And NIH particularly, as an agency that funds 50 percent of university science in this country, and pays most of the indirect costs, that this—(Laughter)—this agency is left out of most of the high-level debate on science and technology in this country.

Mr. BOUCHER. Well, it's a very intriguing response and I thank each of these witnesses for their very helpful suggestions.

The gentleman from New York, Mr. Boehlert.

Mr. BOEHLERT. Two observations before I get to the questions.

One observation. Chairman Brown asked if there would be a level of funding that would satisfy the scientific community, and I was hoping that you would all say in unison "No", because I don't even want the scientific community to be satisfied with the level of funding. I want them constantly reaching and exploring and demanding.

And the second observation—this should bring some comfort to all of you. Dr. Massey, you pointed out that there are 42 authorizing and appropriating committees of the Congress which complicate your daily lives. If there's anything that's needed in this whole equation, it's for us to clean up our act. And the good news is that there's a bipartisan effort, the Hamilton-Gratison Joint Committee on the Reorganization of Congress, that's moving forward at a very rapid rate. There are 284 separate entities of the House and Senate—standing committees, select committees, subcommittees, special committees, you name it—and a disproportionate share of your very valuable time is spent before all these committees getting all sorts of different answers. So we're moving in the right direction.

Now, having said all that, Dr. Healy, I am going to get a transcript of your response to the Chairman's remarks because I could not agree more with it. As a matter of fact, it took this committee to introduce the previous Director of the National Science Foundation to the Secretary of Education. This committee is the one that made that introduction possible. What a sad commentary that is in the way we do business in Washington, D.C. We're talking about intellectual capital and the need to produce the future scientists of America, and we didn't even have the Secretary of Education talking on a regular basis with the Director of the National Science Foundation.

So I think FCCSET is the solution, and I think we've got in Dr. Bromley a preeminent scientist, someone who really wants to make the process work, unlike some of the previous Science Advisors to the President—and you named one—who was nothing more than a cheerleader for SDI. That was his job. Dr. Bromley is vitally concerned with scientific research and a whole wider arena. I think FCCSET, on a day-to-day basis, as middle level functionaries getting together, are talking about things. It's not the policy makers, and at the risk of offending a great deal of the public, I would suggest that Dr. Bromley get the key people together and go off to some retreat for a week. Now, you'd have to suffer all the slings and arrows of outrageous fortune because of suggestions of perk, but how refreshing it would be for having you talking to each other.

Now let me get to some specific questions. Dr. Healy, how extensive is international collaboration in scientific research in your particular area?

Dr. HEALY. Well, we have the Fogarty Center, which is one of our 20 institute centers and divisions, which has the responsibility for our international programs. The budget is in the range of about $20 million. In addition, we support roughly another $20-25 million in specific programs through the individual institutes that are targeted in particular areas of science or focusing on particular diseases. So all together, our portfolio of international activity is approaching $50 million.

Mr. BOEHLERT. Which is really petty cash. I mean, I don't mean to view it that way—

Dr. HEALY. Well, the second part of my comment was going to be that 80 percent of our work is basic science, and that serves the international community. We do the basic science for many of our industrialized partners. We do science that helps the health of this world, not just the health of this Nation. So that you have to look beyond that earmarked money for international programs to the broader issue of the international community that I do believe we serve in our process of discovery for health.

Finally, we have a very large exchange program on our NIH campus, in which foreign visitors, thousands of them are there on a short-term basis and a long-term basis, and get their training and go back to their own countries, and that isn't factored in specifically.

Mr. BOEHLERT. Dr. Massey mentioned the key three letters in this town that drive so much of our activity—OMB. Do you feel, the three panelists, that when OMB is looking at—is in the process of developing the next budget, they look at the broad category of science and then, when they're considering your budget, Dr. Healy, they are also factoring in what Dr. Massey wants, and when they're considering his budget, they're factoring in what NASA needs, and when they're considering NASA's budget—In other words, are they looking at it in totality, or in isolation?

When you walk in from NIH, you talk about the $9 billion public enterprise—I like that phrase—are they just saying well look, you had $9 billion last year, this year, you know, you can get $9 billion plus six percent, or do you think they're looking at the much broader picture? They could help a great deal in this priority setting.

Dr. HEALY. Well, let me answer it from two perspectives. First, this OMB, this particular OMB, from our perspective at NIH, has been extraordinarily supportive and sensitive to the issues of life sciences research. It is unprecedented, but I'm the first NIH Director to have meetings, direct budget meetings, with Mr. Scully and Mr. Grady, and I am expected to have a meeting with Mr. Darman. That is supposed to be arranged through the Department. Typically, the NIH Director never dealt directly at the higher level of OMB, and I think that is a statement of their belief that this is a priority.

But the second is the budget reality, and that is, as NIH is placed—and I think appropriately placed—within a health agency, because that is such a strong part of our mission, we tend to compete on budget lines with domestic programs like Head Start. It's very difficult to say which is more important, Head Start or the NIH. They're both vitally important to this country.

We are not compared with the space station or SSC or the NSF or DARPA. We are not compared with the other science initiatives, even though we're kind of a hybrid. We're both a socially responsible, medically-driven organization that worries about the health of the public, but we're also very important to the scientific underpinnings of this country. On that side, we are not looked at in the global sense.

Mr. BOEHLERT. Dr. Massey, did you want to respond to that one?

Dr. MASSEY. Yes. OMB has worked very closely with the Foundation over the past several years, as well as this committee, and as you know, it resulted in a long-term commitment to double the NSF's budget. So we've had very close interaction with the examiners and with the leaders of the office. And we're now going through a planning process that I mentioned to plan for the outyears, towards the year 2000, and we will be working very closely with them, as we are now, because the commitment to doubling the budget has ended.

That process I feel is being given a great deal of respect. We are putting our priorities in order. We know we're going to have to make strong arguments for the kind of commitment that we'll be seeking, both internally and externally. So at least in my experience, being here a year, I've been very pleased with the process.

I can't speak to how OMB itself works internally and compares across agency lines.

Mr. BOEHLERT. Dr. Healy, do you think, for the most part—and I know it's a combination—but for the most part, the scientific research at NIH is dollar driven or budget driven? Science driven, I should say, science driven. I'm getting so messed up with my notes here. But science driven or budget driven?

Dr. HEALY. I do believe it's been too mechanism and budget driven and not enough science driven, and part of that is that we as a scientific community—and I'm faulting all of us, including me—have tended too much to talk to ourselves and speak, as I said, in code language and worry about—using our benchmark, the number of RO-1 grants we support as a sign of success or failure, when, in fact, the emphasis should be shifted—our measure of success or failure should be the science programs, what we achieve in science.

Now, that doesn't mean that that isn't the undercurrent, but I think as we have presented ourselves, too often it's been in terms of being driven by the number of grants. Quite honestly, that sounds like an entitlement program; that doesn't sound like science. It's not the number of grants. I mean, we could give 20,000 grants out a year if we decided to make them one-year grants. I mean, that's an artificial number. But, unfortunately, it has become the "Holy Grail" of NIH.

One of the things we're trying to do in our strategic planning is to have the community shift their focus from the RO-1 mechanism—not that it's not important as a mechanism, but have programs, have science, have end points, have goals drive our activities.

Mr. BOEHLERT. You have goals and you have pretty much timetables. You try to be realistic in your timetables. You know, we frequently, when we're back home in a town meeting, we'll get questions like this: "Gee, remember back in the Sixties, the President said we're going to get a man on the moon by the end of the decade. Well, here we are in the Nineties and we've still got cancer as the number one killer. Why can't we say by the year 2000, by the end of this decade, we're going to cure cancer?"

I know it's not as simple as that, but can we take a couple of top priorities and set a realistic timetable and agree that the problem is of sufficient magnitude that the science will drive the flow of dollars, rather than the dollars limiting the extent of the science?

Dr. HEALY. Well, we believe that the science should be driving the flow of the dollars, and we believe the scientific opportunities in our particular field of life science, at this point in history, is extraordinary.

I think it is unfortunate, though, to try and think of the life sciences activities in terms of a moon shot. I mean, that was a simple goal. You had one place to go. In NIH, we have thousands of places to go. In the area of cancer, we have hundreds of cancers that we're struggling with, and at the same time the environment around us is changing. People are getting older, so the cancers of yesterday are not the cancers we're going to be worrying about tomorrow. New epidemics come up, like AIDS, or the resurgence of tuberculosis. So we have thousands of places to go, thousands of paths to take, and our challenge is to try and put that within a coherent framework that it allows us to march ahead on all of these fronts. It's much more like a complicated military campaign than it is a single focused moon shot. Would that it were so simple.

Mr. BOEHLERT. One last question.

You talked about—and I couldn't agree more with your Hoffer quote, which I think is great—where you have the power to shape the future, and the way to shape the future is to have the scientists that we need in those laboratories and all over America tinkering and exploring and producing. The growth of intellectual capital is critically important, and Dr. Massey, you pointed out that our training and education for the next generation is just not measuring up. The next century is less than 100 months away, and if you really want to know where my concern is, my fear for the future, I don't worry about some big, bad bomb dropping on the United States and obliterating us; I worry about all those guys around the world that might be smarter than we are and obliterating us in the international competitive arena. So that leads me to conclude that we've got to start at the beginning.

For example, Dr. Massey, I think there should be a national goal, a science priority, that every youngster in America is computer literate by sixth grade. I don't know if that's a realistic goal, but I think it makes some sense, if you look at it. And then I say to myself, how are they going to be computer literate if they don't have computers, so we've got to put some money into that area. Can you give me some comfort in terms of the education aspect of this whole equation on what you're doing at NSF, to begin at the beginning?

Dr. Massey. I hope I can give you some comfort, because the NSF will not solve the entire problem.

Mr. Boehlert. I understand.

Dr. Massey. But we are, I think, making a great deal of progress as a Nation, and particularly within the agency. Over the last five years, as you know, the area of our budget supporting precollege, education, and human resource development has been the fastest growing part of that budget. But it's not just the dollars that have gone into it. We've instituted new kinds of programs, programs that involve the research community that we support, to get them more involved with the education community, both in the universities, and I should say here in Washington.

As you know, we have signed an MOU with the Department of Education, to leverage the resources of both agencies.

Mr. Boehlert. Now that you're talking to each other.

Dr. Massey. Right. Well, you have two new people, so—

Mr. Boehlert. That's exactly right, and I'm comforted by that.

Dr. Massey. We are funding I think some very exciting, not so much experiments, but innovative programs at the precollege level. An example of one relates to your remarks about computers. It shows how we can link some of the research efforts that the Foundation and, indeed, the Nation is supporting with our education effort. We have this large initiative in high performance computing and communication, as you know, which Congress has established in law and came out of the FCCSET process.

To me, one of the most exciting parts of that project is the education and human resource part, whereby as part of the HPCC initiative, is to use the national research and education network to, in fact, reach your goal, to link every school in the country ultimately with each other and with our centers of high performance computing. I don't think it's unrealistic at all to have a goal of every sixth grader being "computer literate" by the year 2000.

Mr. Boehlert. Let me ask you—All right. Let's advance a little bit beyond in a process. A typical college graduate today, bachelor's degree, might be excited about the prospect of a career in science, recognizing that the bachelor's degree is just the entry ticket. You've got to have graduate work, you've got to go all the way to get that Ph.D. if you're really probably going to produce something of significance in the science research arena. And yet, I don't see the adequate funding for fellowships, for example. Can we talk about that for a moment? The typical college graduate today says, "Um, I've got about $15,000 in outstanding obligations because I worked and borrowed my way through school; I'd like to go on to graduate school, but that costs, 20, 25 thousand dollars a year, and I want to get married and start my family and do all these other things." There's not much hope that we're going to get the best and the brightest to go on to graduate school and to continue their studies, so that they will be ideal candidates for your respective Federal agencies unless we provide them the wherewithal to do it.

What are we doing in that area? Let's hear from both of you, Dr. Massey and Dr. Healy.

Dr. MASSEY. Well, we support a number of NSF fellowships, of course, which are—

Mr. BOEHLERT. It's a trend line.

Dr. MASSEY. It's growing. The fellowships are growing. But most of our support for graduate students are not on our fellowships; they're on our research grants, which is something that's often overlooked. We support about 40,000 undergraduate and graduates on a combination of research fellowships and research assistant fellowships. So each time our research budget grows, a large fraction of that research budget is, in fact, supporting graduate students. But it doesn't show up as a separate line for the support of students.

In fact, one might say that one of the co-missions of the support of research by the Foundation is to support the next generation of researchers, because that's the way it's carried out.

Mr. BOEHLERT. Dr. Healy.

Dr. HEALY. Mr. Boehlert, I think that the threat to the intellectual capital base of this country in the sciences is one of the largest threats we face as a country. If you look at NIH, I can give you data that the intellectual capital base which underpins everything that we do—we are no better than the quality of those scientists—is actually contracting. A small amount. But in three of the past four years, the number of principal investigators that we support is contracting. It is not expanding.

Secondly, even more worrisome to us is it is aging. Now, it's not that I have—I support the National Institute of Aging. I believe in aging as a focus for research. But the fact that our brain trust is aging is very worrisome, because it means the young people are not being attracted to science. When they come in, they compete very well, but they are not coming in.

If it weren't for the women that are moving into the field of science, we would see a dramatic contraction in our scientific talent base. In fact, men are decreasing in terms of choosing careers in science and medicine, as women are increasing. Women are compensating for a deterioration in the status of men in the sciences. They are covering over a serious problem. In fact, I think we should worry about the men and wonder where are those smart, young men going, and they're not going into the sciences any more. I think it's a statement of value, and that is my most important point, that the reason why all of those smart, young men aren't going into the sciences is because it's not valued the way it used to be.

I think the heroes of our society are no longer scientists. We don't say one of the goals of this country is to increase the per capita science and engineering numbers. We don't see that. Have you ever heard any political candidate get up and say "we have a crisis, we don't have enough scientists and engineers in this country"? The young people should see that as a goal and should see our scientists and engineers as heroes, and they don't.

Mr. BOEHLERT. Thank you very much.

Thank you, Mr. Chairman.

Mr. BOUCHER. Thank you very much, Mr. Boehlert.

The gentleman from Oregon, Mr. Kopetski.

Mr. KOPETSKI. Thank you, Mr. Chairman.

Miss Healy, I represent Oregon State University and I'm proud of the fact that I like to point out to people back home that the last person I hired for my Federal staff in Washington, D.C. was not a political scientist or a lawyer but a biologist. I tell that to students as well, about how important science is in today's world and how important it will be in their world, especially whether you're on a city council or a state legislative body or even especially in the Federal Congress. The fact is we don't have enough science background to deal with a lot of the social problems that we face here in America today.

I really respect and have come to admire Mr. Boehlert in so many ways. I guess it's a cynical part of me, though, that says how can we become—set a goal of becoming computer literate by the sixth grade for all of our children when we can't even have them be proficient at a third grade level in reading and math by the third grade level. So, in order to get to that computer literacy goal, we're going to have to do something else very fundamental in our society, even before we can address that more specific targeted goal.

I also have had to time to think, I guess. I served on a state legislature and I always wonder why is it that legislative bodies see—or whether at the state level or Federal level, have to try to even consider passing a law to have agencies talk to each other. Why doesn't the executive, whether it's the President or the governor, just call you guys up and say "Why don't we have a meeting on a regular basis", and more than just meet but really communicate and share. Why doesn't that happen? Are you folks so big and is OMB so driven by numbers and budgets that you don't have the time to make this sharing possible?

Sure, and Dr. Massey as well.

Dr. HEALY. I just think that it is more efficient to institutionalize those communication pathways. I think that if you don't have them institutionalized, then you might meet. And as I said, Dr. Massey and I have met a few times over the past year, mainly because we made each other's acquaintance while we were on PCAST some time before that, and because one or the other of us took the initiative to just sit down and talk. But there is not an institutionalized mechanism that would force the head of the National Science Foundation, the head of the NIH, to sit down together, whether they knew each other before they came into their job, or even were so inclined to do that afterwards. So I tend to believe that you have to create the channels, institutionalize those mechanisms, or they are just not likely to occur because of how busy everybody is within their own domain of responsibility.

Mr. KOPETSKI. Dr. Massey.

Dr. MASSEY. I think it's a matter of time, mostly, and priorities in terms of how you allocate the time. I've been making an effort—I think we all have—to meet with individuals. We've had a series of meetings with the heads of the ONR, and AFOSR, the Army Research Office, for example, because their programs are very congruent with things that we support in the physical and mathematical science. We meet with the Department of Education. So it happens, as Dr. Healy said, on the individual basis, drawn by the interests of the agency heads.

I think there's much more coordination at the next level, however, than might be apparent from what you're hearing here. I know the head of our—the Director for Biological Sciences—meets very often with her counterparts at NIH. The head of our Division of Engineering meets very often with the counterparts at DARPA and ONR, and the geosciences, head of Geosciences, close contact with his counterparts at NASA. So it tends, at the level where the research is actually being coordinated and where programs are being developed, I think there's a great deal more there because we're dealing with the same communities. We're supporting the same scientists. When we support astrophysicists, they're the same scientists that NASA supports, and when we support geosciences, they're the same ones that are support in NOAA. So at that level there's more coordination than might be apparent.

At the policy level, at the top, there's room for improvement.

Mr. KOPETSKI. But you're not necessarily in agreement that you would like us to do you a favor and tell you to have this regular meeting, institutionalize it, as Dr. Healy said?

Dr. MASSEY. I think that's right.

Mr. KOPETSKI. Let me move on to a different area.

Dr. Healy, would you rather compete against the space station and DARPA than Head Start, in terms of your budget?

[Laughter.]

Dr. HEALY. If it was a fair compete. I mean, if we really could lay things on the table and have a good, broad, honest reckoning of it, yes. But I don't think, in some vague, quiet, secret way.

Mr. KOPETSKI. So is that saying that the playing field is not level and that you have an advantage now over Head Start and over other human service programs?

Dr. HEALY. No, no, I'm not saying that. I'm saying that just the way our magnificent Government has grown up, it hasn't been—I mean, it has just grown up that NIH has this particular position within the Federal Government and it has managed to do very well within that framework. I personally think that it did better years ago when, in fact, there was more flexibility in the budget, and when there was a greater emphasis on social programs, because NIH was viewed as part of a social agency as much as it was a science agency.

I think that the system has not grown up which enables it to appropriately get an honest reckoning within the world of scientific priorities. Now, that is probably not the case for the other science agencies, but I think it's a peculiarity of NIH. I'm not faulting it.

This is sort of a clinical observation. I'm not saying it's bad or good, and I think NIH has managed to do well within that framework. But I think that, in reality, for NIH to be competing with Head Start does not make as much sense as for it to be competing with another domestic science program. That's all. But I'm not recommending a change. I think to tamper with what is working very well should be done very judiciously and carefully. But I think it's healthy to ask these questions.

Mr. KOPETSKI. Yeah. Well, you brought up a number of interesting areas. Let me ask—I serve on the Immigration Subcommittee of Judiciary, and one of the things I've learned over there—I'm a new Member, by the way, so I'm learning a lot—is our immigration policies are set so that we, in a sense, steal the brain power from other countries. That's one of the other ways that we're making up for the fact that we're not educating scientists in our own country, so that we give a preference to other nations scientists if they want to immigrate to the United States, which I think causes a brain drain out of their country. They lose leaders out of their own country because they're the educated people. But that's a whole different issue.

I was mindful of the comment about women in the society. There is also a critique that's been released recently, I think through Congresswoman Schroader and Miss Oakar, about the fact that our research dollars are not going to those kinds of health issues that affect women, and that there's been a bias in our research for male-oriented health problem or, you know, leaving our women and women's health needs in research.

Are you familiar with these—

Dr. HEALY. Very well. It's been a big issue for me.

Mr. KOPETSKI. Good. So what are you doing in this planning process to affirmatively make up, if you will, through your strategic plan, this fact?

Dr. HEALY. First, I would be very happy if I could submit for the record a summary of the framework for the strategic plan, and you'll see in there we have identified specifically under science initiatives and areas that need emphasis and priorities the whole area of women's health and minority health. We believe that those are research areas that have been neglected, and we also believe that the other side of women and minorities is that they also—that their talent contributions to science have been somewhat neglected, that we need to create more opportunities for women and for minorities to move into the sciences.

I personally believe that one of the most optimistic things about a career in science is that it's brain-driven and not brawn-driven, and as a result, women should be able to compete extremely well in the sciences and in any area of predominantly intellectual pursuit. But I think that we have not yet leveled the playing field so that women can compete and particularly advance within the sciences, for a lot of reasons—and I could take up the rest of the hearing with all the theories, and you don't want to hear them, I'm sure. But nevertheless, at NIH we have a major strategic effort addressing the issues of both women and minorities, both from a research perspective and from a talent development perspective.

Mr. KOPETSKI. Would you characterize that as a significant step forward in terms of affirmatively trying to make up this—

Dr. HEALY. I think it's a significant beginning.

Mr. KOPETSKI. Okay. And it's the most you can do in terms of reestablishing priorities?

Dr. HEALY. Oh, no. I think if you look at our strategic plan—I mean, that's not NIH's only priority. We have identified 10 or 12 areas within science that we view as of critical importance to the science and life sciences base in this country, which include molecular biology, structure biology, the neurosciences, biology and the environment, which I think is an area of growing importance to this country, as well as to the health of the population.

We also have identified a variety of areas that we consider of strategic importance that are more in the policy arena. For example, the social, legal and economic implications of NIH's activities, technology transfer, the development of the talent base in this country, the infrastructure of science as it relates to NIH, issues that have to do with stewardship of our public resources and cost management. So if I could submit for the record the outline, I think you'll see that our strategic plan really is covering a broad range of issues. Women and minorities happen to be one important part of that.

Mr. KOPETSKI. Can I have a final question, Mr. Chairman? Thank you.

You mentioned intellectual capital, and I was—isn't there—I believe there's a qualitative difference also between just beyond the numbers in terms of Japan and Germany, in terms of where they're putting their scientists. We are putting well over half, as I recall, of our scientists into defense related activity. They obviously are not.

How do we—With the changing world, what kinds of incentives can we put out there to attract scientists, some of which may be out of a job so it might be real easy, from the defense industry into other science areas in the service arena? Dr. Massey, if you would like to comment on this as well.

Dr. HEALY. I think it gets back to the broader and tougher issue of national values. I know when I was growing up as a child in New York City, that Bronx High School was science, Hunter College High School, which emphasized the sciences, were all viewed as great aspirations. I mean, this was the post-Sputnik era; this was the time when we saw that the national security of this country and the national sense of heroism was linked to our accomplishments in the sciences.

I don't see that sense of national value today that we had then, that we had after World War II, and I think that does exist in many of these other countries like Japan and like Germany. I give credit to them and I ask us why we are taking for granted our science and technology base in this country. I think the trouble is, because of the long pipeline, we may lose it before we begin to really appreciate it.

Mr. KOPETSKI. Dr. Massey?

Dr. MASSEY. I think, with respect to the specific issue of attracting more scientists and scientific support to civilian related activities from defense, that there are a number of things that could be done, and I believe this is a critical point in the Nation's history that is going to allow us to do that.

It seems that now, as we shift more from a national security motivation for broad-based support of much of our research, if we shift that to a recognition that this can help us lead to creating wealth for the country as a whole, which will allow us to do so many of these things, that maybe we won't have to make the hard tradeoffs between Head Start, housing, veterans' affairs, that we make now because the country will be creating a standard of living and a quality of life that can help us make those choices more judiciously.

I believe now science and technology can be transmitted so much more quickly into improving the economic development of the country, that if we can make that a national focus, then we can support the same amount of research but focus it in a different way. It seems to me that this is a historic opportunity for us to do that.

Mr. KOPETSKI. Thank you.

Thank you, Mr. Chairman.

Mr. BOUCHER. Thank you very much, Mr. Kopetski.

The gentleman from Maryland, Mr. Gilchrest.

Mr. GILCHREST. Thank you, Mr. Chairman.

I think probably the science community needs somebody to pound their fist and be the very vocal spokesperson that the scientific community needs. And you're correct when you say we're not focusing on something very specific as landing a man on the moon, but there are hundreds of problems facing human beings that need to be addressed. The only way we're going to do it correctly is through the scientific community.

Just one minor, little—it's not a minor, little aspect of the scientific community—but probably one of the most controversial issues up here in the House is wetlands, and when you talk about wetlands, if you interject science into it, people will laugh at you. People in the community say we have studied that enough. If you say—I thank God they didn't say that about polio in the 1940s—that science is an ongoing adventure that needs constant attention, and that if we can place science back into its proper place in our Nation, that we will continue to be successful and we won't lose ground.

But when we talked about studying wetlands from a scientific perspective on the House floor a few months ago, people said that it was a mischievous amendment, and when we talk about science being mischievous, I constantly think about Frankenstein or an old movie, or a totally misplaced perspective on what science really is. It's sad that the dollar value is placed as preeminent in this country as opposed to knowledge, which is what science is.

I have kind of a narrow question here for my own sake, but it deals with this understanding—and I don't want to beat a dead horse, so goes analogy, to death—it deals with this communication between the different agencies that are doing a full range of scientific study. We're looking at a scientific assessment of whether or not there is such a thing as global warming and what contributes to that, and ozone depletion and what contributes to that and what are the diverse effects, how that impacts human beings with cataracts and skin cancer and deficiency in their immune systems and things of that nature. Then we look at specific studies of skin cancer and specific studies of AIDS and a whole—and even the depletion of rain forests and some of the biological functions of a rain forest and how they can contribute to our understanding of cures for diseases, things found in the natural habitat of our planet.

If we look at the full range of things that are causing the depletion of the general health of the human population, how much communication is there from NIH for AIDS or cataracts or skin cancer, things like that, with those specific scientific communities that are studying these things that happen in the biosphere. So is there a need then on a regular basis, voluntary or regulatory in nature, for people to discuss these broad range of issues that so impact us as a people? I know it's enormously complicated, but it seems to me that—

Dr. Healy, you mentioned to examine a horizontal transcendent plan that will integrate compartmentalized orientation—you know, how do we communicate with each other as a people. So I would just, I suppose, in that kind of lofty, overspoken statement—should we begin to communicate on a regular basis with the different scientific communities to discuss the possible links to all these things? And I would like to just maybe go right down the line, a quick—we can't do this quickly, I guess—just a remark about that.

Dr. HEALY. I think that it is important to communicate, but I think you have to communicate with an agenda and with a focused goal and purpose for getting together, and I think that's the trouble with the hit-and-miss meetings or with just happening to get together and communicating or so-called coordinating. I think there really has to be an agenda, there has to be a purpose for the gathering, there has to be an outcome of the meeting, and there has to be a reason for having another meeting.

I can tell you that when I took over NIH, one of the most frequently heard complaints, among many others, was that NIH was just a discoordinated mix of 20 independent institutes, divisions and centers that never talk to each other. They were all independent, feudal states and that they just never pulled together.

Well, the fact of the matter is, I think that through our strategic planning process one thing that has become evident to me is that there are issues that transcend all of those institutes that bind them together, there are common goals, there is a common mission, and there are issues that we can only address as a community of NIH, as one NIH, not as a fragmented, compartmentalized assortment of 20 components.

I have been extraordinarily heartened by this year perhaps of all of the things, good and bad, that I have experienced. I have been most heartened by the fact that NIH is moving to a sense that we, as a community, are a fate-sharing vessel, that it is the institutes together that make a strong NIH. It is not any one component part, small or large.

I think that that same model can be looked out throughout science on a broader level throughout Government, and I think what we've learned in our NIH experience is that none of the institutes gave up anything. They just got something in return. They have not given up their ability to pursue their own individual disease-oriented mission; they haven't given up their authorities in terms of dispensing grants; but they have been challenged and they have been brought into a dialogue which I think is enlivening, in terms of why we're all at NIH and where we want to go together.

So I think that there is no reason at all why something similar couldn't be done across Government at a policy level, asking the questions: why does this country invest in science; what are the economic implications of this huge investment of $60-70 billion? Are we getting a return on investment? Should we collectively, as a group of science leaders in Government, ask these questions? How do we do it well?

So I think if we get together and communicate over those important, bigger issues, then there will be reason for getting together. But if we just get together to say hi or coordinate or just talk in a noncoordinated way, or without a focus, then I don't think we're going to achieve anything.

Dr. Massey. Well, NSF research and science, by and large, is not done in Washington. NIH has its own laboratories. Research is done by those individuals who are in the field mostly, institutions, carrying out research. In the kind of question you're asking, it is are the people who are doing the research in these areas speaking with each other. Are they working across disciplinary borders; are they looking at global change and trying to understand it from the perspective of the physics of global change, the effects on the biological system, world weather system, and the answer is they're doing a lot more of that than one might suspect. That's where the action is, is where the people are doing research.

Now, the way we try to influence that, and the way we should, is by funding programs that encourage interdisciplinary work, that encourage scientists, engineers, policy makers, to work together. I think that's the kind of thing that we should be doing here in Washington, and it would help if we had coordinated efforts to fund research activities in an interdisciplinary manner that will allow what you're asking to take place. But I wouldn't want you to think that just because the heads of agencies aren't meeting in Washington that physicists and chemists and biologists and mathematicians are not working together on these very important problems.

Dr. Powell. Let me just add that I think we're moving from the general to the particular.

Let me add a specific comment. You mentioned global warming and ozone depletion, and Dr. Massey mentioned global change. Global change is one of the FCCSET identified initiatives. There is broad interagency coordination in those two specific areas. So I think we're moving in the right direction here and that the FCCSET process is providing a good kind of coordination and oversight.

Mr. Gilchrest. Thank you.

Just for the sake of time, we talk about improving the quality of education and funding a program so that everybody in the sixth grade can use a computer and so on. I note NSF can't go like this and everything will change. I'm really heartened to hear that, and I'm heartened by the fact that there's communication with the Department of Education. But I'll just throw this idea out.

As a former teacher, to me—everybody has their own little things, I know—one way to take a good, solid step in the direction of improving the quality of education for the students is a focus on the teacher, to make sure the teacher, on an annual basis, has the knowledge and the motivation to project that information so the kids have it. It's good to focus on the kids; I don't want to take that away.

But especially with the high-performance computer, you're talking about reaching everybody in the country. And on an annual basis, maybe when school's over, or in the middle of school or some time, every single science teacher, biology teacher, math teacher, even history, English, you name it, they could be collected at a local community college or university, or stay right there in their school, if they had a high performance computer, and just receive the latest advances in science and technology and health and you name it. They'd have the tools, on a regular, updated basis, to go back and give that to those kids. I think we would go a long way to improving the quality of education and the foundation for science, because most elementary school teachers got less than they—They have a difficult task. They don't have training in science or math, those kinds of things. A lot of middle school science teachers, and high school science teachers, they get their degree, maybe in ten years they get a master's or they get 30 more credits, that's approved by the school, but they kind of waffle out there in the interior.

Thank you very much for your testimony. Thank you, Mr. Chairman.

Mr. BOUCHER. Thank you, Mr. Gilchrest. Once again, the subcommittee expresses its appreciation to this panel of witnesses for their very excellent presentations this morning.

You've given us a number of items that will provoke our thought, and we will continue to consider those and will perhaps have some follow-up questions for you. So with this subcommittee's thanks, this panel is excused.

Dr. MASSEY. Thank you.

Mr. BOUCHER. We will turn now to our second panel of witnesses this morning—this afternoon now—for the purpose of focusing on priority setting within the research community itself. We are pleased to welcome to the subcommittee today Dr. John Bahcall from the Institute for Advanced Study at Princeton, NJ., and chairman of the Astronomy and Astrophysics Survey Committee, the committee which I might add has taken a very active role in the process of priority setting within that discipline.

We also welcome Dr. Paul Risser, the vice-president and provost of the University of New Mexico at Albuquerque, and past president of the Ecological Society of America, another discipline within which a measure of priority setting has taken place.

Gentlemen, we look forward to your testimony recounting the experiences that have occurred within your disciplines, making general recommendations to us with regard to the extent to which priority setting can and should take place within specific scientific disciplines, and then following your testimony we'll have questions.

We would ask that you keep your prepared opening statements to about five minutes in the interest of time, and without objection, your written statement will be made a part of the record.

Dr. Bahcall, we'll be glad to begin with you.

STATEMENTS OF JOHN N. BAHCALL, INSTITUTE FOR ADVANCED STUDY, PRINCETON, NJ., AND CHAIRMAN, ASTRONOMY AND ASTROPHYSICS SURVEY COMMITTEE; AND PAUL G. RISSER, VICE-PRESIDENT AND PROVOST, UNIVERSITY OF NEW MEXICO, ALBUQUERQUE, NM., AND PAST PRESIDENT, ECOLOGICAL SOCIETY OF AMERICA

Dr. BAHCALL. Thank you, Mr. Boucher. I'm glad to be here. I think I can make my remarks very brief, since our experience is well-known in Washington. I'm glad you cited our astronomical activities because I think they have been a model for other groups.

I have three main points that I would like to make to you informally, and the first is to just give you a feeling of why we were successful in setting our priorities, what we did, and then to convey to you the conviction that has come up as a result of this experience, that there's nothing unique about astronomers, and what we did it could easily be done in other disciplines and across disciplines, and I would like to make a specific proposal as to how you might facilitate us in doing that.

But just to remind you, since I know you're familiar with what we did, what was characteristic of our committee. We had very wide participation among astronomers in this country and internationally. The President's Science Advisor set our context, not only establishing priorities within science in this country but also in the international context, and so approximately 20 percent of all astronomers in this country participated actively in the priority-setting process, and we got advice very widely from staffers in Washington and from agencies and in the executive branch.

We ended up setting absolutely numerical priorities, one ahead of the other, for a very large range of projects ranging from the multibillion dollar projects to projects that were several millions of dollars. We took account primarily of the scientific benefits of the projects, but we also took account of education and of the effects that would accrue to the Nation in terms of economic competitiveness.

Our primary recommendations were, in some respects, surprising to people in Washington, I think. Our primary recommendations were in neither case in space or in ground-based research to give us big new facilities. In terms of ground-based research, we asked for the emphasis to be placed on the infrastructure, and in terms of space we asked for the emphasis to be placed on smaller, faster missions.

The result of this exercise, in addition to coming out with a prioritized list, was that the astronomical community was united within itself about what were most important for future generations, and we made a very large effort, which was successful, I think, in reaching young people who were curious about the discipline and telling them what the important problems were. For us, the benefits were that we made our own choices and that we came to Congress and to the agencies and to the executive branch and told them what they were.

Now, the question I think relevant for your group, first of all, is why were we successful? I think we were successful because all of us who participated—and we all were active scientists; we were not heads of agencies. We were not, with one or two exceptions, heads of observatories. We were people who wanted to get on with our science and were recognized by our colleagues as having contributed importantly to the scientific discipline. But we al felt that this was the right thing to do as citizens, that we should set priorities in a wider perspective. But I think, even more importantly, we were human beings and we did what we did, which was painful, because we felt it was worth our while.

We selected approximately one out of every ten projects that had been put forward by colleagues after many years of development, entire careers based on that. But nine out of ten of those projects were not selected for the list of recommended projects. And we did that because we felt that there was an incentive for our subject to setting absolute priorities, because we knew that when we came to Congress and when we came to the agencies and said we have a realistic set of proposals, and a limited one, which are supported by the entire astronomical community, then we had a better chance of getting those projects through and funded. So the incentive was very important to us. We're just human beings and we wanted to do what we thought was good for our subject.

Another very important asset for us was the prestige of the National Research Council and the National Academy of Sciences. As chair of this Survey Committee, I was able to get the best and the most active scientists, whose judgments were most widely respected in our field, to serve on this committee because it was a committee blessed and established by the National Academy of Sciences. That carries an important prestige asset in our discipline.

So if I can, having reviewed for you why we are successful, may I just touch on the questions which I think motivate this committee.

The first question is, is there anything different between astronomers and the other fields, and I think no. We brush our teeth in the same way in the morning, and we have our orange juice and coffee in the same way. In my view, the process that we went through can work in any field, across any boundaries. It can work between physics and astronomy and between physics, astronomy and chemistry, space and ground. It can involve biology—if the scientists have satisfied two criteria.

First of all, they believe it's worth their while—and I will come to a specific suggestion on how you might help get that across to our community. It's been traditionally true in astronomy because we've seen that we've had success in getting our highest prioritized projects funded if we establish the priorities ourselves. But I think, across boundaries, your committee could help us with that question. So I think the process could work very widely. I think the National Academy of Sciences, the National Research Council, is an important asset that you should keep in mind.

I think we were successful and we found it relatively easy, although it sounds like a terribly difficult job. In the end, we focused on the criteria that were scientific, and although people had careers, entire careers, based upon decisions—and most of the people were disappointed most of the time—we came out of this process united because we share a common commitment to scientific goals; therefore, we were able to focus on the scientific criteria which are objective, and that's how our list was drawn up. So I hope that you will keep, as you move further in your own thinking, keep primary, so far as the scientists are concerned, limiting the requests to scientists for advice to matters upon which scientists can agree and upon which they're expert, which are scientific matters.

Now, one of the questions I know you've been asking is where should the advice be given. The advice, in my view, has been effectively given at all stages throughout Washington, at the agencies, at OMB, at OSTP, at Congress, in the executive branch, and we've also attempted to reach widely to the public articulating the goals of astronomy.

As a focus of one of your discussions, I mention just for amusement that although our study recommended and the National Science Foundation is funding hundreds of millions of dollars worth of research in astronomy, the National Science Board was one of the few agencies in Washington, one of the few groups in Washington, which did not hear a direct report of our activities. The reports to the National Science Board were made in the way that most things reach the National Science Board—through a digested version of the recommendations being presented to the National Science Board by members and employees of the National Science Foundation. Our group would be delighted to make a direct presentation there, but I think you should have in mind that the National Science Board does not work in necessarily the way that you had envisioned in terms of direct input from the outside.

Now, in my view, one thing that is needed to make this process successful, to have prioritized activities carried on in many different disciplines and across disciplines, involving biology and chemistry and physics and mathematics and astronomy, is for the Congress to provide an incentive to do that and, in fact, the financial incentive. Let me make a modest proposal to you as to how you might do that.

Congress wisely sets aside funds in different areas, and says funds in this category will be allocated to activities of this kind. If your committee or the Congress felt that it was important that there be cross-disciplinary, priority setting in science, you could facilitate that by establishing a category of funding which was cross-disciplinary, and you could say we're going to have 10 percent or 30 percent, or whatever it is that the science budget that the Congress will allocate in a given year, will only be given to those projects which have been reviewed and have come out on top in some committee which looked at science in a broader sense, not just the disciplinarian, not just a committee of astronomers or of physicists or chemists, but a committee which would cut across the board. I guarantee you, there would be a lot of screaming about it, but if that was the law of the land, we'd all get together and we'd use our best judgment and we would work as citizens to have the best possible cross-disciplinary science done if you establish the carrot for us.

Someone asked earlier about what is frequently referred to as "pork barrel" projects in science. They are very frequent. I think the Congress could establish a different category of scientific projects by providing us the incentive for cross-disciplinary projects. With a committee that reviewed just the scientific qualities across discipline, you could have a "bread and butter" instead of a pork barrel science program, and it would be guaranteed to get you the best science because you would have competitively the most articulate and effective people in all of the disciplines competing for the bread and butter science projects, not for the pork barrel projects.

I'd be very happy to discuss any of these aspects with you, and I appreciate the opportunity to discuss with you our common concerns.

[The prepared statement of John N. Bahcall follows:]

STATEMENT TO THE SUBCOMMITTEE ON SCIENCE

OF THE

U. S. HOUSE OF REPRESENTATIVES

Washington, D.C.

April 7, 1992

Prioritizing Scientific Initiatives

by

John N. Bahcall

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What are the most important aspects of the universe to explore? What are the best ways to make discoveries in astronomy and astrophysics? These are tough questions because researchers have many different approaches and it is usually not clear, until the most interesting problems are solved, which method will yield the most important results. Individual astronomers present strong arguments for many potential approaches that require federal funding.

We are well into an era of limited research budgets, however, and choices have to be made. Astronomers have recognized that if they do not set their own priorities, then funding agencies and congressional officials will do it for them. Moreover, the process of convincing colleagues of different specialities improves the proposals and provides a broader outlook for the community of researchers.

Astronomers have recently provided some answers to the hard questions of what to fund and, by implication, what to cut. Working under the auspices of the National Research Council, the astronomers have recommended funding for a limited number of initiatives, ranked in order of priority. Only one out of every ten highly promising initiatives survived this rigorous selection.

I will describe, from my perspective as chairman of the committee, how we came to a consensus on these priorities. I hope that an understanding of our experience may provide further support for the results of our study, as well as offer a possible mechanism for others who must make difficult choices at a time when discretionary budgets are limited.

The group charged with setting priorities, the Astronomy and Astrophysics Survey Committee for the 1990s, was established by the National Research Council (NRC) in May 1989, following my appointment as chair in February 1989. The report of the committee was published in March 1991 by the National Academy Press in book form under the title *The Decade of Discovery in Astronomy and Astrophysics.*

The first step was to find an outstanding group of scientists who were willing to sacrifice a significant part of their research time in order to serve on the committee. I spent most of the months between February and May of 1989 talking to hundreds of astronomers about potential members who might serve on the advisory panels of the survey and on the executive committee (hereafter, the survey committee). I also wrote to the chair of every astronomy department in the U.S., as well as to many other prominent astronomers, requesting nominations. I invited each person to whom I wrote to suggest themes and questions for the study. In addition, I wrote to a number of distinguished astronomers abroad asking about astronomical programs in their countries and requesting advice about possible international collaborations. The staff of the Board on Physics and Astronomy of the NRC received and recorded several hundred replies, nearly all of which were thoughtful and substantial; I also received and answered as many as 60 electronic mail messages a day during the peak of the organizational activities. Many of the ideas the survey committee took up were recurrent themes raised in this initial stage of the study by astronomers from the U.S. and from abroad.

The 15 members of the survey committee were nominated by the appropriate committees of the National Research Council and were appointed by Frank Press, the President of the National Research Council. The survey committee contained six members of the National Academy of Science, two Nobel prize winners, and two directors of national observatories. The committee selected the chairs of 15 advisory panels for different subdisciplines, based on discussions with astronomers of different specialities at institutions throughout the country.

The panel chairs and the survey committee selected 300 people for the advisory groups who had a high level of scientific achievement and who also represented different research approaches, different kinds of institutions, and different geographical areas. The committee tried hard to involve women and minority groups, but with limited success. For example, women constituted 9.5% of the membership in the panels, slightly more than the fraction, 8%, of AAS members over 40 who are women, but somewhat less than the fraction, 11%, of women in the total AAS membership.

Each panel met at a number of different sites in the U.S. in order to help stimulate wide participation by the astronomical community. I also wrote to each of the panel members asking them to solicit the views of colleagues at their home institutions. The survey committee itself considered projects that spanned more than one subfield or which fell between the assigned responsibilities of the panels.

The committee sought and obtained wide financial sponsorship for the survey. In addition to the traditional sponsors of astronomical research—the National Science Foundation (NSF) and the National Aeronautics and Space Administration (NASA)—the survey was sponsored by the Department of Energy, the Department of the Navy, and the Smithsonian Institution, each of which supports some important astronomical or astrophysical research.

Prior to the formation of the survey committee, Frank Press and I visited major agency heads and congressional and administration leaders in order to obtain their advice on what issues the report should address and in what form the results should be presented. I did not ask for support of any projects, but I did hope to create a favorable climate for future consideration of astronomy initiatives. I also did not ask what answers would be politically most desirable. Participants in the survey were encouraged to solicit facts from agency and administration authorities, but we evaluated ideas and initiatives independently and in confidence. Agency leaders, congressional staffers, senior people at the Office of Management and Budget, and the President's science advisor (who had gone through a similar experience as chair of a previous NRC decade survey for physics) all provided valuable advice.

The consultations in Washington resulted in several important sections of the final report: a chapter on the lunar initiative, a chapter on high speed computing, an emphasis on priorities for technology in this decade that will lead to science in the next decade, recommendations of what astronomers should do *pro bono* to help with the crisis in education, a chapter on astronomy as a national asset, an examination of the technical heritage of proposed initiatives, realistic estimates of the costs for each of the new projects, an examination of the role of American astronomy in the international context, some guidelines for assessing when international collaborations would be fruitful, and thumb-nail sketches of major projects that could be used conveniently by those drafting legislation.

Having constituted the panels, the committee's next step was to hold discussions with the astronomical community at large. We felt it was essential to involve the community as much as possible: Every astronomer who had something to say had an opportunity to be heard.

Open discussions were held in conjunction with meetings of the American Astronomical Society (AAS) and at several other professional meetings. In January 1990, at the Washington, D.C. meeting of the AAS, nearly 1000 astronomers participated in open sessions that involved all 15 of the panels. I also wrote a number of short progress reports in the newsletter of the AAS. The names of the survey committee members and of the chairs of the panels were published in the newsletter, along with remarks encouraging individual astronomers to present their ideas directly to survey committee members, panel chairs, or panel members.

The most intense discussions in the first nine months of the survey occurred within the panels. In order to ensure good communication between the panels and the survey committee, each member of the survey committee served as the vice-chair of one of the panels. This arrangement worked well, keeping the survey committee apprised of ideas as they developed and enabling each panel to understand the goals and procedures of the full survey.

The survey committee avoided many potential problems by deciding that the panel reports would be advisory rather than part of the findings of the survey and that the reports would not be refereed by either the survey committee or by the National Research

Council. The recommendations of the panels were not binding on the survey committee, but the panel reports contain important technical information, as well as detailed arguments advocating specific initiatives. The reports of the panels were published separately from, but simultaneously with, the full survey report by the National Academy Press under the title *Working Papers: Astronomy and Astrophysics Panel Reports.*

Establishing the recommendations of the survey took 14 months, about a year less than was projected. The survey committee had six meetings, which took place at astronomical centers throughout the country. At all but the final meeting at which the formal voting occurred, we had open discussions with local astronomers.

I was surprised by one thing. Veterans of similar activities assured me that there would be a difficult and tense period of bargaining before we agreed on the final recommendations. This never happened. I am not certain why. One possible reason is that the committee judged the initiatives on the basis of scientific potential, not political considerations.

The list of priorities was established by a gradual process that was much easier than any of the survey committee members anticipated. The committee voted on straw ballots on three occasions, using as background material the preliminary reports of the advisory panels. The straw ballots focused the discussion on projects that were most likely to be considered important in the final deliberations. As a preliminary to the final ballot, the committee heard advocacy presentations from the panel chairs. The chairs also participated in discussions of the relative merits of all the initiatives, although the final recommendations were formulated by the survey committee in executive session.

Two strategic decisions helped the committee reach a consensus quickly and smoothly. First, the committee decided that if we failed to reach agreement in July 1990 at the pleasant facilities of the National Academy, within reach of the cool breezes from the beach of Irvine, California, then we would meet a month later in the least desirable place in the middle of summer that we could think of, namely, Washington, D.C.

Second, several committee members proposed that I draw up, on the evening before the final voting, a draft list of recommended initiatives in order of priority. They suggested that the committee alter by consensus the draft set of recommendations in order to arrive at the final list of priorities. The proposers hoped that, by this process, the committee could avoid having "winners or losers." I was skeptical of the chances for success when the idea was proposed, but I agreed to try.

Having drawn up a handwritten list of priorities on the night before our formal voting, I was surprised the next day at how rapidly we reached a consensus. We began with those equipment categories concerning which we were most in agreement and then worked our way to the more difficult choices. We went around the table, everyone stating their views about what changes, if any, needed to be made in the ordered list that we were considering. By the time we had all spoken up, the consensus was obvious and we adopted unanimously our priorities in each category.

I was even more surprised that the committee agreed to set priorities that were independent of whether the initiatives were ground-based or space-based, that is, independent of the funding agencies. In preliminary discussions, most agency personnel opposed absolute rankings that combined ground and space initiatives, worrying that their top priorities might be adversely affected by ineffectiveness at some other agency. The survey committee provided both separate and combined rankings of ground and space initiatives, believing that good citizenship required us to use our expertise to provide the maximum possible guidance.

In times of budgetary crisis, good citizenship also requires fiscal restraint. The survey committee studied approximately ten times as many initiatives as were endorsed, recommending that funding agencies invest in astronomical initiatives according to the scientific priorities established in the survey report.

The committee assigned its highest priority for ground-based astronomy to the revitalization of the infrastructure for research, both equipment and people. Continuing to develop a space program with an improved balance between large and small projects, with emphasis on quicker and more efficient missions, was the committee's highest priority for space research.

The report presented a numerically prioritized list of ground- and space-based equipment initiatives in the large- and moderate-sized categories. The committee recommended that an increased emphasis be given in the astronomy research budget to small and moderate programs. The committee did not prioritize small programs, recognizing that the agencies could use peer review for small initiatives to respond quickly to new scientific or technological developments.

Table 1 shows the recommended list of equipment initiatives.

TABLE 1 Recommended Equipment Initiatives (Combined Ground and Space) and Estimated Costs

Initiative	Decade Cost ($M)
Large Programs	
Space Infrared Telescope Facility (SIRTF)	1,300
Infrared-optimized 8-m telescope	80
Millimeter Array (MMA)	115
Southern 8-m telescope	55
Subtotal for large programs	1,550
Moderate Programs	
Adaptive optics	35
Dedicated spacecraft for FUSE	70
Stratospheric Observatory for Far-Infrared Astronomy (SOFIA)	230
Delta-class Explorer acceleration	400
Optical and infrared interferometers	45
Several shared 4-m telescopes	30
Astrometric Interferometry Mission (AIM)	250
Cosmic-ray telescope (Fly's Eye)	15
Large Earth-based Solar Telescope (LEST)	15
VLA extension	32
International collaborations on space instruments	100
Subtotal for moderate programs	1,222
Subtotal for small programs[a]	251
DECADE TOTAL	3,023

The 180 page book presenting the recommendations was written in about three months, with half of the initial drafts of chapters having been produced by the chair or by the executive secretary (Dr. C. A. Beichman of CalTech). The initial drafts of the chapters on science opportunities, on computing, on policy opportunities, and on astronomy as a national asset were provided by the chairs of the corresponding advisory panels. The fact that the executive secretary was a respected scientist who wrote well and shared responsibility for preparing and following-up on drafts was invaluable.

Members of the survey committee revised each chapter extensively. I also solicited informal reviews of individual chapters from 40 distinguished astronomers who represented a variety of institutions and subfield expertise. I was astonished by the number of important revisions that resulted from these informal reviews, both in content and in style. Even after

15 members of the committee had struggled over each word in the draft of a chapter, the informal reviewers suggested significant revisions that were obvious improvements.

National Research Council reports are reviewed carefully. They must meet high standards of logic, of evidence, and of objectivity. In our case, the National Research Council selected 18 formal referees, in addition to a report review committee. The reviewers were anonymous National Academy members and other qualified scientists, in physics, in astronomy, and in other related disciplines. The formal review process was painful, but I answered each review comment, even rhetorical questions, with a specific written response in order that we could complete the review quickly. The 18 referees helped to sharpen our arguments and to clarify our logic, but did not suggest revisions of our priorities.

This is the fourth in a series of decade surveys by astronomers, led, respectively, by A. Whitford, J. Greenstein, and G. Field. The highest priority initiatives in each survey were successfully undertaken, encouraging astronomers to submerge parochial interests and focus on the most important initiatives.

Would another committee of astronomical experts have recommended a similar set of priorities? I think so, provided that they had also spent a year learning about and comparing all the proposed initiatives in this country and abroad.

In summary, these are the things that worked for us: enlisting as committee members active research scientists eager to finish the job and get back to their own work; recruiting an effective executive secretary; insisting on adequate budgeting and staff support; having a logical plan and a specific timetable for completing the report; listening to everyone who wanted to be heard; concentrating on issues within the committee's competence, in our case, scientific priorities; having a talented editor who could sharpen the final report; and working with a community that believes it is better for astronomers to make imperfect judgments about priorities for astronomy than it is to leave the decisions to Washington administrators.

Mr. BOUCHER. Thank you, Dr. Bahcall. We'll have some questions for you momentarily. Dr. Risser, let's hear from you first.

Dr. RISER. Thank you, Mr. Chairman, members of the subcommittee.

I appreciate the opportunity to come and speak this afternoon about a priority-setting project which occurred through the Ecological Society of America. The Society has about 6,500 members, mostly professional ecological scientists. The result of that effort is "The Sustainable Biosphere Initiative." That's in your material as attachment A.

The Society concluded that there were two reasons for the need to set priorities: first of all, that there are clearly more opportunities for research than there are resources; second, however, was a far more important reason, and that is a recognition that, in fact, the biosphere has an enormous amount of threat to it, that without being an alarmist, there are clearly environmental issues not only in this country but around the world which simply have to be addressed. Since those issues are largely ecological issues, the ecological community has to come forth and state very clearly what the highest priority should be for research.

As we begin this process, there were a number of principles that we thought were important. First of all, that the best scientists ought to be involved in this process; we ought to involve as many members of the Society as possible in the process; that the recommendations should not be a single approach to science but rather, in fact, should preserve the multiple approaches that have been so successful in science in this country's scientific enterprise; that we should not seek to perpetuate the current subdisciplines of this science but, in fact, look across the disciplines, not only in ecology but other disciplines as well; that we ought to select the top priorities from a number of options—that is, to be selective; that the scientists themselves ought to make the initial recommendations; that the Federal agencies need those recommendations but they ought to come straight from the scientific community; that we also ought not to have the current structure of the Federal Government constrain the kinds of ideas that come forth; and lastly, and perhaps most importantly, that we maintain the criteria of science excellence throughout all these decisions.

The Ecological Society selected two criteria for making these judgments. First of all, the contribution to the fundamental knowledge in ecological sciences. What we discovered is that the fundamental nature of ecological sciences has, in fact, made enormous contributions to our understanding of environmental systems and ecological systems. We have to preserve that emphasis on the fundamental nature of ecological research.

Secondly, however, was the recommendation and the recognition that, in fact, ecology ought to focus on problems which are important to society and particularly to the sustainable biosphere.

Now, I pause here for a second because that is an enormous change in this scientific community. Heretofore, its decisions have been largely made on the basis of scientific merit, on the interest of intellectual curiosity. But the Ecological Society at this point says that the biosphere is so important that we simply have to step back and devote our resources to that particular issue. So it's a change in culture, it's a change in thinking, it's a recognition by this scientific community of what really is important in terms of our priorities.

As we thought about those priorities, we also recognized those instances in which the scientific community is ready to make a contribution—that is, it's a readiness—and in those instances where we think scientists can actually solve problems in the near term as well as the longer term.

Now, the priorities, when we got to the point of actually identifying them in this process, were based on two different directions. First of all, to recognize those intellectual frontiers which were most important in the views of the scientific community, in those areas where the science is moving rapidly, in those areas where the best scientists think we have the best chance for being successful. We also recognized that in selecting those environmental problems they ought to be the ones which are, in fact, the most pervasive in this country as well as elsewhere. So we looked at the priorities not only from the intellectual frontiers but also from the importance of the environmental issues themselves.

The result of looking at those two criteria—that is, the importance in the scientific sense, as well as the problems to society—came forth with three priorities. Here are the three:

First, global change. The broad issue of global change has been discussed this morning in many different directions. It clearly is an issue which brings together many disciplines. One of the areas, however, which has been neglected is, in fact, the role of ecological processes in global processes; that is, the role, for example, in vegetation and how it changes the flux of greenhouse gases or trace gases into the atmosphere, how global change temperatures and precipitation patterns affect the biosphere itself, again the effect of vegetation. So the emphasis here is on the ecological processes which play such an important role in the global processes in the general global change program.

The second of the three priorities is in the area of biological diversity or biodiversity. Much of the effort in this country in biological diversity has focused on either cataloging species or preserving pieces of land. Both of those are important and laudable objectives. What's missing, however, in the research program of this country is the way in which that biological diversity affects these same ecological processes that I've just talked about and the way those ecological processes affect biological diversity. So the second priority has to do with the relationship between the biological diversity on this planet and the way in which ecological processes are affected by that diversity and affect the diversity itself.

The last of the three priorities is a far more aggressive and ambitious one, and it focuses on how we sustain ecosystems for the near term and also for the long term. Now, this is an objective which crosses many agency boundaries. It's a very complex topic because it includes, for example, how we manage crops, how we manage species which are currently of commodity value, as well as those who are not currently of community value, how we manage many species in the same place when each of those species have different requirements and different expectations, and finally, how we make these decisions across broad landscaped units. It's not enough for us to learn about a single forest or a single cropland or a single grassland; it's how, in fact, those parts of the landscape interact and how we manage those interactions.

The first two already have some programs at the Federal level. That is, there is, as you know, a global change program. There are some programs in biodiversity. A coordinated effort on sustainable ecosystems does not exist in this country among the Federal agencies, although most agencies have some part of it. So the third priority is going to require a new structure, a new coordination mechanism, and it will, in fact, drive the Federal agency to look at this problem in a much more coordinated way than any issue we've talked about heretofore.

Now, why was the Sustainable Biosphere Initiative a successful one? I can try and summarize the reasons why I think this process has worked so well. First of all, it was a deliberative process which took two years, and yet it moved along so rapidly that it maintained an enormous amount of momentum and interest in the scientific community as well as here in Washington. The leadership in this project were, in fact, scientists with the highest credentials—that is, the scientific credentials. Thirdly, there was an open and communicative process with the Society as a whole. In fact, about 20 to 25 percent of the Society participated in this process at one stage or the other. The fact that the process focused on problems of the Society, as I have indicated, was, in fact, a new orientation and one which I think was extremely important and captured the imagination not only of the ecologists but also of associated scientists in other disciplines. The contributions to basic science were clearly important as we built on this record a recognition about how that basic science can now be applied to applied problems became clearer and clearer throughout this process. The recommended three priorities were convincing of their own right; that is, they were scientifically convincing, but they were also convincing because of their importance to society as a whole.

It's also important to recognize that this advice was essentially independent of the Federal agencies; that is, it maintained a certain separateness identified by the scientific community, so the advice comes to the Federal agencies as essentially independent advice from the best of the scientists.

However, lastly I would like to comment on what's happened to the biosphere initiative once it's been completed and to show you why it seems to be important.

The ultimate objective, of course, is to have available the best ecological information for managing the biosphere so it can support this generation and future generations, and that's the ultimate test of the sustainable biosphere initiative.

But in the mean time, several steps have occurred which I think are important for the deliberations of this subcommittee. First of all, about a dozen Federal agencies met on two separate occasions together—that is, representatives from a dozen agencies—came and sat and talked about this particular initiative, talked about how it fit in their agencies and what their objectives ought to be vis-a-vis this particular initiative, and subsequently there have been numerous discussions with individual agency representatives and also from the FCCSET Committee on global change.

Secondly, we have established now in Washington an office to support this sustainable biosphere initiative. This is funded by five agencies and it's put in place so the agencies can continue to obtain advice and information from this particular initiative.

We are also in the process of establishing an interagency committee of these agencies and others which will have a continuing discussion with the sustainable biosphere initiative to make sure the project becomes important not only in its own right, but also becomes incorporated in the budget initiatives of the various Federal agencies. There are a number of workshops which are being planned to refine this particular initiative. There was a recent one held, for example, with the National Park Service in which the future research agenda of the Vegetation and Wildlife Division of the National Park Service was planned in the context of the three priorities of the sustainable biosphere initiative.

There have also been meetings with other professional societies, other disciplines, in some cases because those disciplines wish to endorse the sustainable biosphere initiative, and in some cases because they wish to work collaboratively in the future steps.

Also, the sustainable biosphere initiative has had recommendations and input to the National Academy of Sciences. As some of you may know, there is a committee on environmental research now in the National Academy of Sciences. That committee is to help design ways in which environmental research ought to be conducted in the Federal agencies, and this particular initiative has been an input to that process.

Lastly, I should say that the sustainable biosphere initiative, or as it's commonly known, the SBI, has now become international. There was a meeting set in Mexico this last summer which endorsed the international sustainable biosphere initiative, and in Spain in January the International Special Committee on Problems in the Environment took that initiative and has now taken the international sustainable biosphere and it's now become an international program as well.

So, Mr. Chairman, what I hoped to have done this afternoon is to convey to you the process used by the Ecological Society for setting priorities, to give you a summary of what those priorities are and what the criteria were used in arriving at those criteria, to tell you what's happened subsequently, and to tell you why I think the process was so successful.

Thank you.

[The prepared statement of Paul G. Riser, with attachments, follow:]

Testimony to the

U.S. House of Representatives

Science, Space and Technology Committee

Subcommittee on Science

HEARING ON PRIORITY SETTING IN SCIENCE

by

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April 7, 1992

THE SUSTAINABLE BIOSPHERE INITIATIVE:

AN ECOLOGICAL RESEARCH AGENDA

Ecological Society of America

Good Morning Mr. Chairman, Members of the Committee.

I appreciate the opportunity to discuss the topic of setting research priorities, particularly as this subject has recently been addressed by the Ecological Society of America. Just last year, the Ecological Society completed a major report that clearly established research priorities in the field of ecological sciences. This report, THE SUSTAINABLE BIOSPHERE INITIATIVE: AN ECOLOGICAL RESEARCH AGENDA, is enclosed as Attachment A and a shorter summary as Attachment B.

The Ecological Society of America is a professional society, begun in 1915, currently with 6,500 members located primarily in this country but also in many other countries around the world. Most members serve as academic scientists in colleges and universities, but others are found in local, state and federal organizations and in the private sector. The scientific discipline of ecology is quite broad, encompassing subjects as diverse as the genetics of colonizing plants or animals in croplands and tropical forests to the movement of nutrients and greenhouse gases to and from entire watersheds. Research approaches in this science include highly theoretical and mathematical models, sophisticated laboratory techniques for measuring such characteristics as the behavior of organisms or minute quantities of pollutants, and measurements of greenhouse gases across whole watersheds using remote sensing imagery and aircraft-based sensors.

In discussing the research priority-setting process used by the Ecological Society, I will offer the following topics: why priorities should be set, the criteria that should be used in setting priorities, the specific criteria used in the Sustainable Biosphere Initiative, the research priorities identified in the Sustainable Biosphere Initiative, how the stated priorities are being used in the federal research enterprise, and finally, a summary of why the Ecological Society of America was successful in setting research priorities for the next decade.

Why Ecological Research Priorities Must Be Stated

There are many examples of environmental problems that lead to the unequivocal conclusion that human activities have begun to threaten the ability of the Earth to support even current lifestyles around the world (1, 3). Examples of degradation of ecological systems include increased problems with the disposal of solid and toxic waste, rapid rates of deforestation and watershed destruction, high rates of species extinction caused by human activities, and changes in tropospheric trace gases and in weather patterns. All of these problems involve ecological processes, such as cycles of species populations, reduction in the availability of habitats, and exchange of CO_2 and other gases by the vegetation. Thus, many if not most of these environmental problems are fundamentally ecological in nature. Ultimately, there is simply no science priority higher than maintaining the biosphere in such a condition that it can support this and future generations.

Because so many of the world's most challenging problems must be addressed by ecological science, in 1988 the Ecological Society of America concluded that research priorities must be set. There were two reasons for this decision. First, the Society recognized, as did National Academy of Sciences President Frank Press and others (2, 4), that there are always more research opportunities than there are resources to support the research. Second, since sustaining the biosphere is such an overwhelming mandate, ecologists as an entire profession have the distinct responsibility to state the research that is most important and should be of the highest priority.

Even agreeing to set research priorities at all was a significant, momentous and unusual step for this large and diverse professional society. If one examines the history of science, it is clear that much of the United States' highly successful research effort is attributable to the intellectual curiosity of individual scientists pursuing stimulating questions without having specific or immediate applications in mind. Indeed, to retain the vitality of the ecological sciences, this investigator-initiated research approach must continue to be vigorously supported and nurtured. Here the criteria for setting priorities are the judgements of scientific merit and importance as well as estimates of the capabilities of the investigators.

This philosophy of using scientific merit as the single criterion for setting research priorities is well inculcated in the ecological sciences and has served the profession well. In fact, nothing in this discussion nor in the conclusions of the Sustainable Biosphere Initiative countermand the importance of this approach and it should remain as a key part of ecological research.

Since the federal research effort is limited by resources and because maintaining the biosphere is of utmost importance, the Ecological Society began the process of setting research priorities. In establishing this process, several operating principles were considered to be particularly important:

1. Policy and management decisions must be made about the Earth's resources. These decisions will be best if they are based on the most complete knowledge available; therefore, strong scientists should assume a leadership role in the process of setting research priorities.

2. Scientists should make the initial recommendations about research priorities because it is they who recognize the practical and theoretical directions that are most likely to be fruitful, know the ideas and approaches that are moving most rapidly and successfully, know both the opportunities as well as the scientific obstacles for applications of the results, and understand the status of the existing technologies and facilities.

3. The federal research enterprise must make priority decisions, but cannot be expected to do so in a rational manner without the explicit advice of the scientific community.

4. Recommendations on the highest priority research topics should not be restricted to one federal agency, nor should the current configuration of the federal agencies and their program definitions constrain the form or type of research recommendations.

5. In setting priorities within the scientific discipline, many possibilities must be considered and evaluated, but only the highest priorities must be presented in the final recommendations.

6. As many members of the scientific discipline should be involved as possible and the process itself should be open to encourage the best ideas and to entertain all recommendations.

7. The recommendations should not focus on equipment or on single approaches, but rather should continue the emphasis on research ideas and concepts, and should encourage multiple approaches to solving ecological questions and problems.

8. The recommendations should not simply perpetuate the current subdisciplinary emphases, but should seek to identify and synthesize priorities that benefit from treatment by the entire breadth of the field of ecology.

9. The criterion of scientific quality and merit should not be compromised.

<u>The Methods Used by the Ecological Society of America in Developing an Ecological Research Agenda</u>

The process used by the Ecological Society consisted of several sequential steps:

1. In the Spring of 1988, a Steering Committee was established, chaired by Dr. Jane Lubchenco, Chair, Department of Zoology, Oregon State University. This five-person Steering Committee developed the general outline of the process and wrote proposals to secure the initial funding for the project. The full fifteen-person membership of the Research Agenda Committee was also defined and the appointments were made (see authors, Lubchenco et al. 1991 as Attachment A).

2. At the annual meeting of the Society in 1988, the Research Agenda Committee met for an open initial discussion about the expectations of the effort and the criteria to be used in selecting priorities.

3. During the next one and a half years, the Committee met four times. At one of the meetings, several experts in selected areas were invited to join the Committee in reviewing draft material.

4. Throughout the process, there were multiple invitations for input, through letters sent to Sections and Chapters, to individual researchers, calls in the Bulletin and Newsletter, and informal contacts with members of the Research Agenda Committee.

5. Prior to the 1990 annual meeting, drafts of the report were sent to anyone who requested a copy. At this meeting, a general discussion of the draft report was held with about 900 attendees. Also, there was an open "town hall" in which any person could come and make comments and recommendations. All of this advice was recorded.

6. During the following months, additional written comments were received from more than 100 persons. Also, the draft report was sent to all Society Sections and Chapters, the Public Affairs Committee and the Executive Committee.

7. The Committee met the last time to incorporate the advice from the Society and complete the report which was approved by the Society Executive Committee. The report (1) was then printed in Ecology, the primary journal of the Society (Attachment A).

Initial discussions of the Research Agenda Committee focused on the criteria that should be employed in making recommendations, the appropriate number and breadth of the priorities to be recommended, and the format of the final report. After the criteria had been established (see next section), many different alternatives were considered as possible research priorities. Thus, the final recommended priorities were the result of discarding many possibilities and selecting only the most important.

Criteria Used in Selecting the Research Priorities in the Sustainable Biosphere Initiative

Two primary criteria were used to identify the research priorities:

1. The potential to contribute to fundamental ecological knowledge

2. The potential to respond to major human concerns about the sustainability of the biosphere

The first criterion is important because basic research is the foundation on which informed environmental decisions must rely. Otherwise, decisions are likely to be problem-specific and lead to unsuccessful applications under different circumstances. Moreover, research on fundamental ecological processes has proven to be extremely effective in addressing very applied issues (e.g., natural history of insects has led to the basis of integrated pest management techniques; understanding the physiology of algae has led to understanding aquatic pollution and eutrophication).

Acceptance of the second criterion is an enormously important statement from a scientific community which has heretofore simply set priorities on the basis of individual intellectual interests. Stating that responding to major human concerns should determine research priorities is a clear recognition by the ecological community that sustaining the biosphere is such an important objective that research must be focused on this issue.

Identification of Priorities

Establishing the actual priorities based on these criteria was approached from two directions: Intellectual Frontiers and Environmental Problems.

Intellectual Frontiers

Employment of the concept of intellectual frontiers means that the best available science, as we know it today, was incorporated into the identification of the most important research priorities. Pragmatically, using intellectual frontiers means that the recommended research involves the search for general principles that can unite disparate studies and provide a basis for extrapolation and prediction. This results in significant research efficiencies and power because, for example, lessons learned from one organism or habitat can be tested for application elsewhere. Since the science identifies the actual mechanisms, extrapolation and prediction are based on a consistent understanding of the underlying ecological processes. With these products of ecological research (new empirical results, new conceptual advances, new research tools), useful general principles can be identified and employed to manage the biosphere.

Environmental Problems

Selection of research priorities was also based on those problems that were ecologically most severe, *and* on those problems in which research had a reasonable opportunity of providing answers. So the selected priorities were based on the perceived severity of the environmental problem, the feasibility of the research, and the readiness of the scientific community to provide resolutions and recommendations.

Research Priorities of the Sustainable Biosphere Initiative

Using the two primary criteria and the approach of identifying specific priorities on the basis of both the intellectual frontiers and the severity of environmental problems, the following three research priorities were identified in the Sustainable Biosphere Initiative:

Global Change: understanding how ecological processes affect local, regional, and global climate conditions, and how changes in those patterns of climate affect ecological processes. These many interactions involve the atmosphere, soil, and water and may be driven by, for example, changes in climate and in land use.

Biological Diversity: understanding how patterns of genetic, species and habitat diversity are affected by human activities and natural phenomena. In particular, ecologists need to understand how diversity affects the behavior of ecological systems and how ecological processes control biological diversity.

Sustainable Ecological Systems: understanding when natural and managed ecological systems are stressed to the point that they are no longer capable of being sustained, how to restore damaged systems, and how to manage ecological systems so that they can remain productive to support natural processes and the human population.

There is an ongoing large national and international research program on global change. However, especially in the United States, far too little attention has been paid to the ways in which ecological conditions and processes control global processes. The distribution patterns of organisms, as well as differences in the rates of vegetation productivity and storage of chemicals, all influence global processes such as fluxes of greenhouse gases between the

Earth's surface and the atmosphere, transfers of materials from terrestrial to aquatic habitats, and many other biosphere functions. For example, as global concentrations of atmospheric CO_2 increase, certain groups of plants would be expected to become stronger competitors and, as a consequence, achieve a more dominant position in the Earth's vegetation. Thus, changes in these ecological processes might lead to decreases or increases in the productivity of forests, rangelands or croplands. Similarly, management practices and policies might result in changes in forestry practices which in turn may influence the amount of CO_2 or precipitation in the region.

In the general topical area of biological diversity, relatively little attention is currently devoted to understanding the ways in which different levels of diversity control how the biosphere will respond to climate change, or how this diversity affects the rate at which water and nutrients move across the landscape into rivers and oceans, or how different kinds of ecosystems will respond to pollution. For biological diversity to be protected, it will be necessary to understand how ecological processes (e.g., seasonal dynamics of precipitation and available nutrients such as nitrogen) influence biodiversity. If, for example, there is a reduction in the number of species in a rangeland or forest, there is the possibility that these ecosystems may be less productive, and also less able to resist the consequences of air pollution or changes in weather patterns.

These first two research priorities represent added ecological contributions to research areas in which there is currently significant research activity. The third priority of evaluating and managing ecological systems represents the need for a major new integrated research program. This priority is designed to prescribe the most effective restoration and management strategies for ensuring the continuation of the Earth's ecological systems. Emphasis will be placed on research which will consider commodities which do not currently have a market value, on the consideration of several commodities simultaneously, and on understanding how to restore and manage relatively large landscapes and regions. This research agenda topic is at the heart of most of the nation's most vexing resource management issues. For example, management and policy decisions must be made that depend on the best trade-offs between forest products, fisheries, and recreation within an entire region. The best long-term decisions will be based on an understanding of what is required to maintain the supporting ecosystems, how these ecosystems interact at regional levels, and how these ecological systems can be restored to provide natural services for human consumption.

These three priorities represent important ecological processes and severe environmental problems. Fortunately, in many cases, specific research studies can provide information that will be required to address two or even all three priorities. Their ultimate resolution will depend upon the concerted effort of many associated disciplines, thus a multidisciplinary approach will be required.

Although not emphasized in this presentation, the Sustainable Biosphere Initiative recognized two additional components. Right now, the best ecological information is often not being used to make policy and management decisions and there is widespread ecological illiteracy among the public; these are extremely serious deficiencies in the availability and management of ecological information. With this lack of information or the use of less-than-the-best ecological information, decisions are made that in turn cause other environmental problems and also prevent us from making the best possible decisions. First, the structure of and emphasis on ecological education must be enhanced. The new generation of ecologists must be proficient in several disciplines to address these priorities. Moreover, ways must be developed to ensure that the public can understand these ecological issues as well as the consequences of various public choices about the management of natural resources. Second, ecological information must be made available in ways that decison-makers can incorporate it into rational judgements that will contribute to the sustainability of the biosphere. Ensuring that this knowledge transfer process works well and efficiently will require more sophisticated approaches to information communication.

Implementation of the Sustainable Biosphere Initiative research priorities will require cooperation among several federal agencies. The funding requirements have been estimated by two approaches : (a) comparison with other large-scale projects such as the human genome project and (b) a systematic analysis of the current research programs of all the pertinent federal agencies to determine which parts of the Initiative are now funded but require additional funding and which parts of the Initiative are not currently included in existing research programs. Both methods of estimation indicate that as an absolute minimum, an additional $500 million annual budget will be required for U.S. research by the fifth year, and the total Initiative will require approximately ten years to complete.

Impacts of the Sustainable Biosphere Initiative

Although the Sustainable Biosphere Initiative has been published for only one year, a number of impacts have resulted from the Initiative. These are summarized below:

1. On two occasions, representatives from about a dozen federal agencies met to discuss the Initiative, to suggest ways in which the Initiative could be implemented, and to propose ways that part of the Initiative could be incorporated into their program and budget initiatives. In several cases, portions of the Initiative research priorities became a part of the agency's budget request. There also continue to be numerous discussions with representatives of many agencies about the Initiative.

2. With funding from several federal agencies, the Sustainable Biosphere Project Office has been established in Washington. This Office, staffed with a full-time senior ecologist, a research assistant and support staff, is designed to facilitate the implementation of the Initiative. Major activities of the Office include the organization and encouragement of workshops on special topics, particularly to refine the research priorities and to provide information to federal agencies about the Initiative.

3. An interagency committee has been established to oversee the development of the Sustainable Biosphere Project Office and its various activities. This committee is comprised of representatives from those agencies who are sponsoring the Sustainable Biosphere Project Office.

4. Workshops with sponsorship from several agencies have been held or planned for the purpose of providing additional detail about some of the research priorities. For example, one workshop was sponsored by the National Park Service with the purpose of planning the Service research agenda for vegetation and wildlife in the context of the Sustainable Biosphere Initiative. Another workshop sponsored by several agencies is designed to bring the social sciences into the research plans for the sustainable systems research priority.

5. Representatives from several other professional societies have met to discuss the Sustainable Biosphere Initiative, to consider ways in which these societies can assist in the implementation of the Initiative, and in some cases, to evaluate whether these societies should undertake a similar effort to define research priorities in these disciplines.

6. The Sustainable Biosphere Initiative has provided advice and recommendations to other organizations in Washington, such as the Committee on Environmental Research of the National Academy of Sciences.

7. The Sustainable Biosphere Initiative was used as a model in developing an international Sustainable Biosphere Initiative (A Sustainable Biosphere: The Global Perspective, published by the International Association of Ecology, Ecology International 1991:20). Subsequently, at the Scientific Committee on Problems of the Environment (SCOPE) General Assembly in Seville, Spain, the Sustainable Biosphere Project was adopted as a SCOPE project and an initial planning committee has been appointed to begin the implementation of this international program. The international Sustainable Biosphere Initiative has also been communicated to ASCEND 21, and will be presumably be a part of the scientific input to UNCED later this year.

Reasons Why the Sustainable Biosphere has been Successful

The ultimate success of the Sustainable Biosphere Initiative can be measured only after several years when the research programs have provided successful prescriptions for maintaining the biosphere, conserving natural resources and increasing the quality of life in this country and throughout the world. However, as indicated above, the Initiative has begun to be successful in assisting the federal agencies and these agencies have sought advice from the Initiative. Other professional societies have agreed to endorse the Initiative and in some cases to consider conducting similar priority-setting activities. And the Sustainable Biosphere Initiative has played the major role in spawning an international Sustainable Biosphere Project.

The Ecological Society of America has now begun to analyze the reasons for the success of the Sustainable Biosphere Initiative, especially since this effort will need to be revisited as the science unfolds during the next decade. The following conclusions seem particularly important:

1. The entire process was led by scientists with strong research credentials and who were dedicated to the process.

2. The process itself was open and communicative so the entire Society had the opportunity to be involved. Indeed, a large proportion of the Society membership (about 20 to 25%) was involved. The intact Society structure was mobilized to assist in the effort by reviewing draft reports and providing ideas and recommendations. Also, there was continuous coverage of the Research Agenda Committee activities in newsletters and bulletins so the Society as a whole felt involved in the process.

3. Despite a history of largely resisting the notion that setting research priorities would enhance the quality of science, in the face of insufficient resources, the Society recognized that research priorities must be set and they should be set first by the scientific community.

4. The Society as a whole recognized the need to focus research on societal problems and this criterion was considered in parallel with the more traditional values of basic science.

5. The field of ecology has demonstrated that basic ecological science can successfully address societal and national needs, thus permitting a focus on those research areas of the largest environmental problems.

6. The recommended three research priorities (global change, biological diversity and sustainable ecological systems) were convincing on their own scientific and societal merits.

7. The research priorities were not only scientifically strong, but they provided needed advice to the federal agencies who profit from clear advice from the scientific community. This advice is most valuable to the federal agencies if it comes with strong support from the professional society, in this case, the Ecological Society of America.

8. The process itself occurred over a long enough period of time to be credible, but fast enough to maintain momentum and interest.

9. Sufficient funding was available to provide support to the Research Agenda Committee and for the publication of the report (Andrew W. Mellon Foundation, Ecological Society of America, National Aeronautics and Space Administration, National Science Foundation, Oregon State University, U.S. Department of Energy and the U.S. Environmental Protection Agency).

10. The Initiative has been useful to the federal research program (as indicated by the impacts after only one year), but the priorities came directly from the scientific community with no direct influence from the agencies. This separation of advice from any actions taken by the recipient agencies increased the credibility of the recommendations and enhanced the confidence of the scientists stating the priorities.

I hope that this description of the process used by the Ecological Society of America in constructing the Sustainable Biosphere Initiative has been helpful to the Science Subcommittee.

Thank you.

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RISSER TESTIMONY
Attachment A

The Sustainable Biosphere Initiative: *An Ecological Research Agenda*

A Report from
The Ecological Society
of America

Ecology, 72(2), 1991, pp. 371–412

THE SUSTAINABLE BIOSPHERE INITIATIVE: AN ECOLOGICAL RESEARCH AGENDA

A Report from the Ecological Society of America[1,2]

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Key words: biological diversity; biosphere; ecological research; environmental decision-making; global change; research agenda; research funding; research priorities; sustainability; sustainable ecological systems.

PREFACE

This preface introduces a document that is unprecedented in its scope and objectives. In August 1988 the Ecological Society of America initiated an effort to define research priorities for ecology in the closing decade of the 20th Century. Several independent factors motivated this endeavor. First, within the academies of science, the halls of government, and the institutions that fund research, it had become increasingly clear that scientists must order their priorities and make hard judgments concerning the research directions that hold the greatest promise for advancing our base of knowledge and for improving the human condition. Responding to this need, Frank Press, the President of the National Academy of Science, challenged all scientists to define their priorities. Financial resources are finite. Competing national demands range from national security to social services, and various major priorities vie for attention and funding. Consequently, it is not feasible to support all scientific research. If we as scientists do not set our own priorities, others will do so for us.

Second, the need to ameliorate the rapidly deteriorating state of the environment and to enhance its capacity to sustain the needs of the world's population has become paramount. We will increasingly require ecological knowledge to utilize and sustain the Earth's resources. Although the needs for new knowledge and for the application of existing knowledge are increasing, the means to accomplish these goals are decreasing due to the limitation of available funds. Tough decisions need to be made concerning what to fund and what not to fund.

Against this background it is essential to make clear that basic research is the foundation on which informed environmental decisions must rely: the greater are the applied needs, the more important becomes basic research. If this point is not made clear, narrowly based applications will carry the day. Unless the science of applied ecology is based on a sound foundation, attempts to manage the environment are bound to fail. The greatest advances in ecological understanding have come from the creative fertility of investigators, carrying out basic research motivated by intellectual curiosity. It is critical to examine how best to nurture the development of this basic substructure, and to train the ecologists of tomorrow.

The dilemma of increasing needs in the face of decreasing means, and the challenge to identify priorities, set the stage for the Ecological Society of America to lead its members into a period of introspection, in which the whole realm of ecological activities would be examined. The present study is the centerpiece of that analysis. It identifies those endeavors that were deemed most ur-

[1] The authors listed serve as members of the Ecological Society of America's Committee for a Research Agenda for the 1990's. Institutional affiliations can be found on page 405.

[2] Address reprint requests to: The Ecological Society of America, Public Affairs Office, 9650 Rockville Pike, Suite 2503, Bethesda, Maryland 20814 USA.

gent in terms of both the advancement of the field and the potential for improving the human condition.

In order to accomplish this monumental task, one of us (HAM) established a broadly representative committee, under the leadership of Jane Lubchenco, then Vice-President and now Second President-Elect. This committee, composed of ecologists representing a wide array of ecological subdisciplines, met intensively over a period of more than a year. They undertook to identify the most exciting and relevant areas of ecological research and to submit their conclusions to the critical evaluation of the Society's membership and other interested parties. Their efforts included consideration of research priorities, needs in education and outreach, and strategies for implementing the recommendations.

The process of review and revision has been one of the most thorough any document has ever received. Although this effort was led by committee, the document itself truly represents input from the entire Ecological Society of America, and from a broader community as well. Early on and throughout the process, calls were made through the *Bulletin* of the Society, through the Public Affairs Office newsletter, and through workshops and seminars for input on the document and the process itself. These calls resulted in the involvement of large numbers of people, and the incorporation of their ideas. In August of 1990, at the annual meeting of the Society, a presentation of the draft document was made to nearly a thousand members. There was widespread support for its sense and structure. Questions from the floor provided further input as did a subsequent workshop, also attended by a large number of Society members. Following the annual meeting more than 150 letters were received from members giving further suggestions. This new input was incorporated by the committee into the document that follows. Although individual Society members undoubtedly would not agree with every detail of this report, the iterative review and revision have resulted in a document that is a community-wide effort of which we can all be proud. The Executive Committee of the ESA has enthusiastically endorsed this report. We cannot rest too long on the success that we have achieved. The challenge that faces us is to make the program outlined here a reality, and to include our international colleagues and those in related disciplines as partners in this bold undertaking, to provide the scientific basis for a sustainable biosphere.

The committee is pleased to acknowledge the Andrew W. Mellon Foundation, the Ecological Society of America, and Oregon State University for support for conceptual development of the SBI, and the Andrew W. Mellon Foundation, the National Science Foundation, the U.S. Environmental Protection Agency, the National Aeronautics and Space Administration, and the U.S. Department of Energy for support of publication costs. We (HAM and SAL), gratefully acknowledge the leadership efforts of Jane Lubchenco, and thank the committee for its remarkable efforts.

H. A. Mooney
President 1988–1989
Ecological Society of America

and

Simon A. Levin
President 1990–1991
Ecological Society of America

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EXECUTIVE SUMMARY

In this document, the Ecological Society of America proposes the Sustainable Biosphere Initiative (SBI), an initiative that focuses on the necessary role of ecological science in the wise management of Earth's resources and the maintenance of Earth's life support systems. This document is intended as a call-to-arms for all ecologists, but it also will serve as a means to communicate with individuals in other disciplines with whom ecologists must join forces to address our common predicament.

Many of the environmental problems that challenge human society are fundamentally ecological in nature. The growing human population and its increasing use and misuse of resources are exerting tremendous pressures on Earth's life support capacity. Humankind must now develop the knowledge required to conserve and wisely manage Earth's resources. Citizens, policy-makers, resource-managers, and leaders of business and industry all need to make decisions concerning the Earth's resources, but such decisions cannot be made effectively without a fundamental understanding of the ways in which the natural systems of Earth are affected by human activities. Investigator-initiated, peer-reviewed basic research is the foundation on which informed environmental decisions must be based. Ecological knowledge and understanding are needed to detect and monitor changes, to evaluate consequences of a wide range of human activities, and to plan for the management of sustainable natural and human-dominated ecological systems.

In response to these national and international needs, the Ecological Society of America has developed the Sustainable Biosphere Initiative (SBI), a framework for the acquisition, dissemination, and utilization of ecological knowledge which supports efforts to ensure the sustainability of the biosphere. The SBI calls for (1) basic research for the acquisition of ecological knowledge, (2) communication of that knowledge to citizens, and (3) incorporation of that knowledge into policy and management decisions.

Research Priorities

This document focuses primarily on the acquisition of ecological knowledge. It identifies the ecological research programs of highest priority and recommends the steps required to pursue

research objectives. The document also lays the groundwork for improving the communication and application of ecological knowledge.

The criteria used to evaluate research priorities were (1) the potential to contribute to fundamental ecological knowledge, and (2) the potential to respond to major human concerns about the sustainability of the biosphere. Based on these criteria, the SBI proposes three Research Priorities:

- **Global Change,** including the ecological causes and consequences of changes in climate; in atmospheric, soil, and water chemistry (including pollutants); and in land- and water-use patterns
- **Biological Diversity,** including natural and anthropogenic changes in patterns of genetic, species, and habitat diversity; ecological determinants and consequences of diversity; the conservation of rare and declining species; and the effects of global and regional change on biological diversity
- **Sustainable Ecological Systems,** including the definition and detection of stress in natural and managed ecological systems; the restoration of damaged systems; the management of sustainable ecological systems; the role of pests, pathogens, and disease; and the interface between ecological processes and human social systems.

Research Recommendations

Each of these research priorities requires a different type of action. Existing national and international initiatives address aspects of the first two priorities. However, the success of these programs will require increased emphasis on key ecological topics.

RESEARCH RECOMMENDATION #1: Greater attention should be devoted to examining the ways that ecological complexity controls global processes.

Within the topic of global change, insufficient attention has been paid to the ways in which ecological complexity controls global processes. Such key factors as species and habitat diversity, patterns of distribution of ecological assemblages, and differences in the productivity and storage capabilities of different types of ecosystems all influence how the biosphere functions in the Earth system.

RESEARCH RECOMMENDATION #2: New research efforts should address both the importance of biological diversity in controlling ecological processes and the role that ecological processes play in shaping patterns of diversity at different scales of time and space.

Within the topic of biological diversity, much of the current effort is devoted to enumerating the species in various habitats and to preserving biotically significant sites. These important efforts lay the groundwork for the research proposed here and must be continued, but two vitally important topics must also be addressed. First, it will be necessary to discover to what extent patterns of biological diversity are important in determining the behavior of ecological systems (e.g., responses to climate change, rates of nutrient flow, or responses to pollutants). Only when these relationships are known will it be possible to develop management strategies for maintaining natural and human-dominated ecological systems. Second, it will be necessary to understand how ecological processes interact with physical and chemical factors to control or determine biological diversity. Doing so will require investigation of the manner in which individual species interact with and are modified by the abiotic environment on both ecological and evolutionary time scales.

RESEARCH RECOMMENDATION #3: A major new integrated program of research on the sustainability of ecological systems should be established. This program would focus on understanding the underlying ecological processes in natural and human-dominated ecosystems in order to prescribe restoration and management strategies that would enhance the sustainability of the Earth's ecological systems.

Plans for comprehensive programs in the areas of global change and biological diversity are more advanced than those in the area of sustainable ecological systems. Research programs exist to develop specific sustainable natural resources (e.g., sustainable forestry or sustainable agriculture). However, current research efforts are inadequate for dealing with sustainable systems that involve multiple resources, multiple ecosystems, and large spatial scales. Moreover, much of the current research focuses on commodity-based managed systems, with little attention paid to the sustainability of natural ecosystems whose goods and services currently lack a market value. Addressing the topic of sustainable ecological systems will require integration of social, physical, and biological science.

Implementation

Successful implementation of the SBI will require a significant increase in interdisciplinary interactions that link ecologists with the broad scientific community, with mass media and educational organizations, and with policy-makers and resource-managers in all sectors of society. This document recommends specific actions that will begin to develop such links and initiate the first steps of the SBI. The action items that follow will be initiated by the Ecological Society of America, but will require broad support and participation by other groups and individuals, ranging from federal and state funding agencies and other scientific societies to policy-makers, leaders of business and industry, and concerned citizens.

Research component of the SBI

Initiation of the research component of the SBI will involve coordination with ongoing programs as well as initiation of new programs. A series of workshops is proposed to bring ecologists together with experts from related disciplines in the natural and social sciences and with resource-managers and environmental policy-makers to develop projects for immediate action.

ACTION ITEM #1: During the coming year, an organizing committee of the Ecological Society of America will plan workshops with the goal of coordinating the SBI with current research efforts on global change and increasing research on the role of ecological complexity in global processes.

ACTION ITEM #2: During the coming year, an organizing committee of the Ecological Society of America will plan workshops with the goal of developing an initiative on biological diversity that focuses on the ecological causes and consequences of patterns of biological diversity.

ACTION ITEM #3: During the coming year, an organizing committee of the Ecological Society of America will plan workshops with the goal of initiating a comprehensive program on sustainable ecological systems, emphasizing the underlying ecological processes that affect the sustainability of natural and managed systems.

Education component of the SBI

The environmental conditions that have mandated the Sustainable Biosphere Initiative also demonstrate the need for ecological education among citizens of today and tomorrow. Understanding and managing the biosphere requires ecological information. There are many strategies for addressing educational needs, such as working with the mass media to increase public awareness of ecological concepts and issues, making ecological literacy a goal of undergraduate curricula, and developing more interdisciplinary graduate degree programs that involve topics necessary for understanding the biosphere. The following action items represent the first steps in addressing these needs.

ACTION ITEM #4: During the coming year, the Research Agenda Committee of the Ecological Society of America will oversee the preparation and publication of a non-technical, public education document that articulates the importance of ecology and ecological research to society.

ACTION ITEM #5: During the coming year, the Education Section of the Ecological Society of America will develop systematic, short- and long-term strategies for enhancing ecological knowledge among students and the public.

Moreover, the Ecological Society of America should determine the human resources needed to conduct the ecological research proposed by the SBI and should develop specific vehicles to address the identified needs, including training grants and career development awards.

Environmental decision-making component of the SBI

Thousands of ecologically based decisions are made annually by policy-makers and regulatory agencies, land- and water-use planners, resource-managers, business and industry, consulting firms, and conservation groups. To be useful to decision-makers, ecological information must be both accessible and relevant to their mandates and responsibilities. Therefore, the application of ecological knowledge will require better communication between ecologists and decision-makers in all sectors of society. The experience of management-oriented professional societies in setting environmental priorities will be essential to open new avenues of communication.

ACTION ITEM #6: During the coming year, an organizing committee of the Ecological Society of America will begin to explore ways in which ecologists can become more responsive to and bring their expertise more fully to bear on critical environmental problems. This committee will work closely with management-oriented professional societies, resource-managers, and other environmental decision-makers.

International dimensions of the SBI

The framework for this Initiative was developed in North America, but the research priorities and the environmental problems related to them are important world-wide.

ACTION ITEM #7: During the coming year, the Ecological Society of America will organize a meeting of leading ecologists from many nations of the world to evaluate the SBI and to begin construction of an operational framework for international cooperation.

At the same time there will be efforts to interact with governmental (e.g., UNESCO) and non-governmental (e.g., the International Council of Scientific Unions, ICSU) international bodies that have programs closely related to the research agenda of the SBI.

Funding the SBI

Meeting the financial needs of the SBI will require significantly increased funding from both public and private sources. Because of the broad importance of this Initiative, creative approaches to funding research will be required. Typically, public agencies such as the National Science Foundation fund basic research, mission agencies fund research that applies to problems of specific interest to the agency, businesses fund research to answer pressing industry questions, and foundations fund topics or themes of particular interest. The SBI encompasses all of these missions, and as a result, must be planned and funded by a range of agencies and organizations.

Current administrative structures are insufficient to coordinate and fund the range of activities envisioned by the SBI. Consequently, it will be necessary to develop a new administrative structure that allows many agencies to support the integrated research program. To accomplish the needed coordination and funding, a variety of vehicles should be considered, including a new or existing interagency committee, a new national institute, or other administrative arrangements. This new organization would further develop research priorities within the SBI, coordinate funding strategies, and establish and implement procedures for evaluating the research progress of the Initiative.

In constructing new interdisciplinary and interagency approaches, it will be particularly important to preserve the opportunity for creativity and innovation. The cornerstone of the SBI should be investigator-initiated, peer-reviewed research conducted by individual investigators or multidisciplinary research teams.

ACTION ITEM #8: During the coming year, the Ecological Society of America will initiate discussions to develop an innovative framework to coordinate and fund the SBI. Emphasis will be placed on enhancing opportunities for investigator-initiated, peer-reviewed research in the context of coordinated programs that would fund both individual investigators and multidisciplinary research teams.

The ecological research agenda proposed in this document begins with the assumption that advances in understanding basic ecological principles are required to resolve many urgent environmental problems, continues with the identification of three priority areas for intense research efforts, and concludes with actions to be initiated by the Ecological Society of America to strengthen and expand research efforts in these key areas. The success of the Sustainable Biosphere Initiative will depend upon (1) the willingness of individual ecologists to participate in the proposed activities, to disseminate the vision of the SBI, and to plan and execute subsequent phases, and upon (2) the vision and abilities of policy-makers, funding agency administrators, government officials, business and industry leaders, and individual citizens to support, amplify, and extend the actions we have initiated. At present, neither the funding nor the infrastructure in this country is sufficient to address the research needs described in this document. Moreover, achievement of a Sustainable Biosphere will require not only the acquisition of ecological knowledge via research, but also the communication of that information and understanding to all citizens and the incorporation of that knowledge into environmental, economic, and political decisions. Although there are formidable barriers to accomplishing these tasks, achieving a Sustainable Biosphere is one of the most important challenges facing humankind today. Time is of the essence. New technologies, widespread appreciation for the magnitude of environmental problems, and an increasing appreciation for the relevance of basic ecological research combine to provide an unprecedented opportunity to make significant progress in achieving a sustainable biosphere.

I. INTRODUCTION

Environmental problems resulting from human activities have begun to threaten the sustainability of Earth's life support systems. Among the most critical challenges facing humanity are the conservation, restoration, and wise management of the Earth's resources. Citizens, policymakers, resource-managers, and leaders of business and industry all need to make informed decisions concerning these resources. Ecological knowledge is one critical facet of the information required for making complex environmental decisions. Ecological understanding and knowledge are urgently needed to detect and monitor environmental changes, to evaluate consequences of a wide range of human activaties, and to plan for the management of sustainable ecological systems. New interdisciplinary connections will be required to conduct the needed research, to educate scientists and the public, and to ensure that the special expertise of ecological science is available to environmental decision-makers in all sectors of society. In response to these national and international needs, the Ecological Society of America proposes the **SUSTAINABLE BIOSPHERE INITIATIVE (SBI)**, a framework for the acquisition, dissemination, and utilization of ecological knowledge to ensure the sustainability of the biosphere. In this document, we define the scope of, and develop the rationale for, this Initiative.

Many of the environmental problems that challenge human society are fundamentally ecological in nature. The human population now numbers 5.2 billion, and is increasing at a rate approximating 1.8% each year. The growth of this population and its increasing resource use are exerting tremendous pressure on Earth's ecological systems. As a result, Earth's life support systems are changing, and their ability to sustain human society is being degraded rapidly. The sustained productive capacity of the Earth is at risk, as evidenced by the increasing difficulties in managing solid and toxic wastes, rapid rates of deforestation and watershed destruction throughout the world, high rates of species extinction caused by human activities, and changes in the atmosphere, such as increases in tropospheric trace gases and depletion of stratospheric ozone. Many environmental problems, particularly those involving hunger, disease, and sustainable resource use involve patterns of resource allocation as well as total resource availability. As the world's population expands, as demands for the reallocation of scarce resources continue, and as developing nations' standards of living change, the effects of human activities on the Earth's resources will grow at even faster rates.

Ecological understanding of complex phenomena is essential if society is to anticipate and ameliorate the environmental effects of human activities. Human activities may have unanticipated or indirect effects on parts of the Earth's life support systems, often at considerable distances from the site of the activity. For example, tropical deforestation may affect global climate by altering the global carbon balance. The introduction of irrigated agriculture may affect the productivity of marine fisheries by the alteration of water quality and flow regimes due to damming. In such

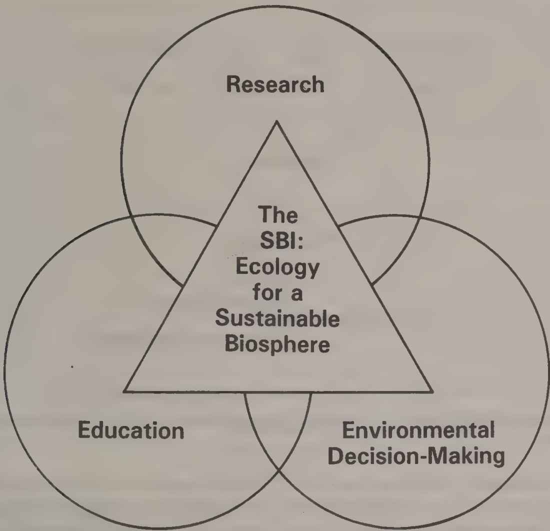

FIG. 1. Interdisciplinary interactions called for by the Sustainable Biosphere Initiative (SBI).

cases, ecological research can elucidate the links among populations, communities, and ecosystems, and between the abiotic and biotic realms.

The establishment of new interdisciplinary connections will facilitate the advancement of ecological understanding and help make ecological knowledge more accessible to the public and to environmental decision-makers (Fig. 1). Advances in the physical, chemical, biological, or social sciences are interdependent. Just as fundamental discoveries in ecology may depend on data or techniques derived from other scientific disciplines, information on the role of ecological processes in the physical or chemical environment, or in social systems, can contribute to advances in other fields. However, the acquisition of new ecological knowledge will be insufficient to address the Earth's environmental problems unless that information can be disseminated and used. In addition to improved programs for teaching ecology in the traditional educational context, increased interaction between ecologists and the media is needed to enhance public awareness and understanding of ecological approaches and principles. Moreover, interactions with environmental decision-makers in the public, private, and non-profit spheres must be facilitated. A forum is needed for discussion of the ecological information most critically needed to solve specific environmental problems and of how best to disseminate ecological information to decision-makers.

Within the field of ecology, the SBI calls for advances in research, improvements in education, and enhanced application of fundamental ecological knowledge in environmental decision-making (Fig. 2). This document focuses primarily on the research component of the SBI. In it we identify the ecological research programs of highest priority and recommend the steps required to pursue the research objectives. The educational and environmental decision-making components of the SBI require further development to identify needs, set priorities, and make recommendations for the communication and application of ecological knowledge.

THE SUSTAINABLE BIOSPHERE INITIATIVE

The research component of the SBI is the primary focus of this document. The criteria used to evaluate priorities for this research were (1)

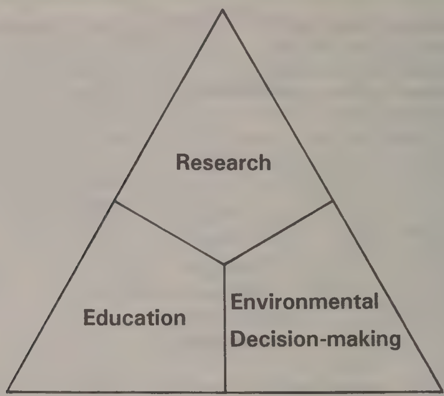

FIG. 2. Components of the Sustainable Biosphere Initiative: the acquisition, communication, and utilization of ecological knowledge.

the potential to contribute to fundamental ecological knowledge, and (2) the potential to respond to major human concerns about the sustainability of the biosphere (Fig. 3).

Based on these criteria, the SBI proposes three Research Priorities:

- **Global Change,** including the ecological causes and consequences of changes in climate; in atmospheric, soil, and water chemistry (including pollutants); and in land- and water-use patterns.
- **Biological Diversity,** including natural and anthropogenic changes in patterns of genetic, species, and habitat diversity; ecological determinants and consequences of diversity; the conservation of rare and declining species; and the effects of global and regional change on biological diversity.
- **Sustainable Ecological Systems,** including the definition and detection of stress in natural and managed ecological systems; the restoration of damaged systems; the management of sustainable ecological systems; the role of pests, pathogens, and disease; and the interface between ecological processes and human social systems.

The last of these three priorities—the sustainability of ecological systems—is one of the greatest challenges facing human society, yet it is the one that has received the least attention to date. We strongly endorse efforts already under way to address problems of global change and biological diversity. Moreover, we call for a greatly accelerated and expanded effort toward developing sustainable ecological systems.

Although ecologists have unique knowledge and skills that allow them to conduct research on these topics, interactions with other disciplines are necessary for a truly comprehensive approach to urgent environmental problems. Studies of global change, for example, cut across many fields, including ecology, atmospheric chemistry and physics, oceanography, hydrology, and geology, as well as human demography and economics. Likewise, to address issues of biological diversity, ecologists must collaborate with taxonomists and conservation biologists, policy-makers, planners, political scientists, and economists. Finally, sustainable human use of Earth's resources will require new alliances between ecology and other disciplines, such as resource management; agronomy, forestry, soil science, and other environmental sciences; epidemiology and demography; economics and planning. Ecology, in many ways an interdisciplinary science itself, will play a critical role in accelerating the development of new interdisciplinary approaches to the study of these environmental problems.

An initiative of the magnitude we envision will transcend traditional institutional boundaries and will involve innovative new collaborative pro-

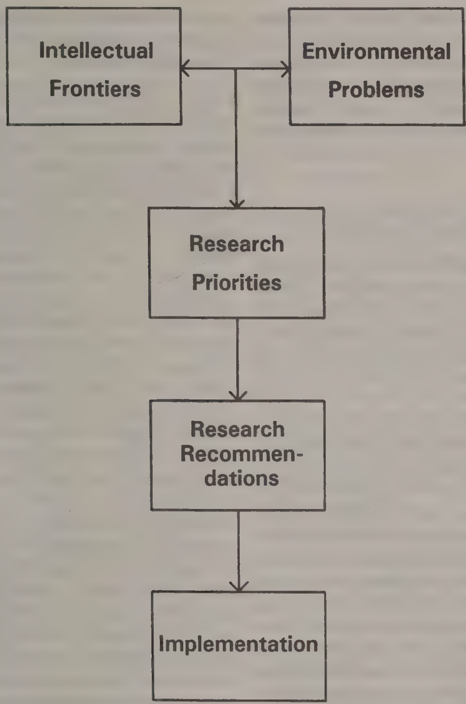

FIG. 3. Relationships among sections of this document. Intellectual frontiers and environmental problems are dual criteria used to establish research priorities. Essential components of research in the priority areas have received insufficient attention. These key components form the basis of the research recommendations. Implementation of the recommendations will require specific actions by the Ecological Society of America and by other supporting institutions (Section VI).

grams. In constructing expanded interdisciplinary and interagency approaches, it will be particularly important to preserve the opportunity for creativity and innovation. Thus, the cornerstone of the SBI should be investigator-initiated, peer-reviewed research. The SBI is not the work for a single agency; interagency cooperation, perhaps through a coordinating committee or a new institute, will be essential to achieving the objectives. Moreover, coordination with national and international agencies and institutions outside the United States will ultimately be required.

The primary message of the SBI is that advances in understanding basic ecological principles are required if environmental problems are to be resolved. The three seemingly distinct priorities—understanding the consequences of global change, understanding and conserving biological diversity, and assuring a sustainable future—share a common, ecological foundation, i.e., an understanding of the structure, functioning, and resiliency of natural systems. This document shows these links and indicates the fundamental ecological research needed to address the priorities. In this document, we explore the ecological principles and questions from which the priorities were selected, i.e., intellectual frontiers in ecology (Section II), and ecological knowledge required to help solve environmental problems (Section III). Subsequent sections highlight the research priorities and key research topics needed to address the priorities (Section IV), the major research recommendations of the SBI (Section V), and an action plan for further developing the SBI (Section VI).

II. INTELLECTUAL FRONTIERS IN ECOLOGY

Intellectual frontiers serve as one starting point (Fig. 3) for identifying research priorities. These frontiers are firmly grounded both in those ecological problems that are linked to specific levels of biological organization (Appendix A), and in those problems that cut across these levels (Appendix B).

Ecology has developed from a largely descriptive science to one that also includes analytical, experimental, and comparative approaches, and employs sophisticated laboratory, field, and remote sensing techniques. A growing body of ecological theory addresses the principles that govern the regulation and organization of populations and communities in space and time, and the interaction of biotic and abiotic components of the environment. New tools, including remote sensing, computational approaches, molecular and isotope analyses, and global-scale models, afford the opportunity to discover new ecological phenomena and to study known phenomena at previously inaccessible spatial and temporal scales. (See National Research Council 1989*c* for a more comprehensive treatment of new opportunities in ecology.)

In this section, we present an overview of interesting, exciting questions in ecology, arranged from individual- and evolutionarily based questions to those involving the interplay between the biotic and abiotic components of ecosystems. Several criteria were used in the decision to highlight these specific research questions. First, these questions are synthetic. They involve a search for general principles that can unite disparate studies and provide the basis for extrapolation and prediction. Second, these questions represent frontiers in ecology, because new empirical results, new conceptual advances, and new research tools hold the potential for clarifying general ecological principles. Although we have identified these intellectual frontiers based on their potential to advance the science of ecology, we also point out their obvious applications in the solution of environmental problems.

• **What are the patterns of diversity in nature, and what are their critical ecological and evolutionary determinants?** Understanding the diversity of nature is, in various forms, a fundamental problem of ecological research. New techniques have extended the temporal and spatial scales over which patterns of diversity can be detected. Modern molecular techniques permit systematists to construct phylogenies based on genetic material or population biologists to analyze the fine-scale genetic characteristics of existing populations. These techniques open new possibilities for describing the evolutionary history of diversity and elucidating the mechanisms that regulate genetic variation in modern-day populations. Remote sensing technologies are increasingly used to describe large-scale patterns of diversity at the community, ecosystem, and landscape levels. Characterizing patterns of diversity is a critical first step in preserving that diversity, hence providing the foundation for conservation biology. In community ecology, one of the most active areas of empirical research and conceptual synthesis is the elucidation of how abiotic and biotic factors interact to generate patterns of diversity. There is a growing need to conduct theoretical and empirical studies aimed at integrating mechanistic explanations with large-scale patterns of diversity. Understanding what regulates diversity is central to guiding strategies for habitat preservation, and for restoration ecology.

• **How do morphological, physiological, and behavioral traits of organisms interact?** Much of classical biology is concerned with the relationship between structure and function. The relationship of the morphology of organisms to the tasks they perform—how they resist physical stresses, how they capture prey, or how they attract mates—is at the core of the study of nature. In the growing field of biomechanics, novel applications of physics and engineering principles and use of new technology have permitted significant advances in understanding the functional costs and benefits of morphological variation in organisms. New applications of stable isotope analyses in plant ecology have the potential to link physiological and environmental processes in new ways.

Modern approaches have succeeded in placing traditional questions within a proper evolutionary framework (e.g., Jacob 1977). Recognition of the importance of frequency dependence has led to numerous recent advances in the application of game theory to behavioral and evolutionary problems. Such perspectives have motivated the development of more sophisticated theories that link systematics, autecology, and evolutionary biology. The next decade should witness the successful application of these approaches to a wide array of problems.

• **How plastic are the morphology, physiology, and behavior of organisms in the face of environmental stresses? What are organisms' proximal limitations?** Understanding the extent to which the genotype of an organism determines its phenotype and the degree to which environmental factors can modify the expressed phenotype is a classical problem (i.e., nature vs. nurture) in biology and psychology. Separation of the sources of variance among genetic and environmental factors was one of the first great conceptual advances of the theory of population genetics. Analysis of plasticity is critical to understanding the capacity of organisms to respond to anthropogenic changes and predicting whether environmental changes will cause genetic shifts within populations and taxonomic shifts within communities.

• **What are the determinants and consequences of dispersal and dormancy?** Dispersal and dormancy are two of the most basic life history responses to environmental variability. They govern the persistence of the majority of species within communities because disturbances of various kinds create colonization opportunities. They also hold the key to the recovery of damaged ecosystems, to the spread of species following climate change, and to the spread of introduced species, including genetically engineered organisms.

• **What factors explain the life history adaptations of organisms? What are the population-level consequences of these adaptations?** The theory of life history evolution is one of the richest branches in evolutionary ecology. Its relation to population-level phenomena (including reproductive tactics, dispersal, dormancy, phenology, resource allocation, and other traits) has been the focus of active research since Lamont Cole's landmark paper (Cole 1954). Game theory and related approaches described earlier have given us a new set of tools to address these problems. The importance of understanding how populations will respond to environmental change has given us new motivation to find answers. Life history theory should be an active area of investigation in the next decade.

• **What factors control the sizes of populations? How are changes in population size related to processes mediated at the level of the individual?** Understanding what controls population dynamics is a central question in ecology and one that also lies at the core of a remarkable diversity of applied issues. These include the management of harvested populations (e.g., fisheries), the spread of agricultural pests and human disease, the persistence of endangered species, the success of deliberate introductions of exotic or genetically engineered organisms, the possible accidental and undesirable spread of those organisms, and restoration ecology.

The mathematical theory of population dynamics, involving periodic and chaotic behavior, threshold behavior, and multiple equilibria, has seen great advances in the past 15 yr. Theories abound, and the challenge is to link these theories to data by relating individual performance and population dynamics. Considerable work is under way on individual-based models aimed at replacing classical phenomenological approaches with mechanistic models that will allow a basis for extrapolation beyond historical experience.

• **How does the internal structure of a population affect its response to various stresses?** The dynamics of a population are affected fundamentally by its internal structure, including its age, stage, and genetic structure, and its spatial distribution. Classical population dynamic theories have tended to view populations as lumped aggregates of identical units, except for the explicit treatment of genetic structure in evolutionary theory. Yet other aspects of population structure have been shown to be critical in understanding coexistence of species, population fluctuations, the spread of disease, and other critical phenomena. In recent years, attention has turned to developing methods to incorporate demographic and spatial structure into population models, setting the stage for important advances.

• **How does fragmentation of the landscape affect the spread and persistence of populations?** Natural and human-induced patterns of disturbance interact with species' traits and interspecific relationships to affect the patterns of spread, persistence, and abundance of species. Understanding these influences has been a problem of fundamental theoretical interest for nearly half a century (Watt 1947). Today, the study of land mosaics plays a key role in efforts to link processes in local populations, communities, and ecosystems with those at the level of the biosphere. Human land use has modified patterns of fragmentation. Because the extinction of desirable species or the spread of undesirable ones may depend in part on landscape patterns, studying these problems has become increasingly urgent.

• **What factors govern the assembly of communities and ecosystems and the ways those systems respond to various stresses? What patterns emerge from cross-system comparisons?** The analysis of patterns of community structure—including description of the trophic network—is a central focus of ecological theory. Numerous theoretical approaches have been used to develop an understanding of the key factors that generate and maintain that structure across a range of temporal and spatial scales. Studies in island biogeography have made a useful contribution by blending theoretical and experimental approaches to the processes governing assembly of communities. Work on these questions must be given increased attention, both for its fundamental theoretical importance and because of its relevance to problems of restoration and recovery of ecosystems following major damage.

Experimental studies have examined how particular ecosystems respond to different classes of perturbations, ranging from nutrient or pollutant additions to the removal of species. Multifactor experimental studies have been instrumental in understanding how biotic and abiotic factors interact to shape communities. These studies have led to an increased appreciation for the role of indirect effects in species' interactions. Such studies form the foundation of community and ecosystem theory. Their scope must be expanded. It is necessary to compare and synthesize the ways different ecosystems respond to a particular class of stresses and the ways a particular ecosystem responds to different stresses. Such studies, in addition to their obvious theoretical importance, can lay the basis for a functional taxonomy of ecosystems and guide research in ecotoxicology, restoration, and management.

• **What are the feedbacks between the biotic and abiotic portions of ecosystems and landscapes? How do climatic, anthropogenic, and biotic processes regulate biogeochemical processes?** Work on this topic must include studies of the exchanges of energy and materials among ecosystems and of atmospheric–biospheric and land–sea interactions. Furthermore, although numerous studies have described the biogeochemical cycles and patterns of energy flow within ecosystems (in some cases across a range of spatial and temporal scales), few mechanistic theories exist to explain how those cycles and flows are regulated. How robust are they in the face of disturbance? What is the role of biota in regulating climate and ecosystem processes? Research on the linkages between the biotic and abiotic portions of ecosystems and between population biology and ecosystem approaches is essential to understanding how those systems will respond to global change, and comprise one of the greatest of challenges facing ecologists.

• **How do patterns and processes at one spatial or temporal scale affect those at other scales?** Recent developments in remote sensing and Geographic Information System (GIS) technologies permit examination of ecological patterns at spatial scales larger than was previously possible. At the same time, there has been increased appreciation for the importance of processes at small spatial sales (e.g., dispersal and recruitment) in the structure of populations and communities. Long-term ecological studies and the development of new techniques to reconstruct past communities and environments have extended the temporal scale of ecological studies, while continuous or frequent sampling has highlighted the importance of small-scale temporal variation. In addition, experimental and observational studies suggest that temporal and spatial scales interact (e.g., rare events may have profound effects on spatial pattern). The increasing availability of data across temporal and spatial scales and the urgency of solving large-scale environmental problems have stimulated theoretical and empirical studies

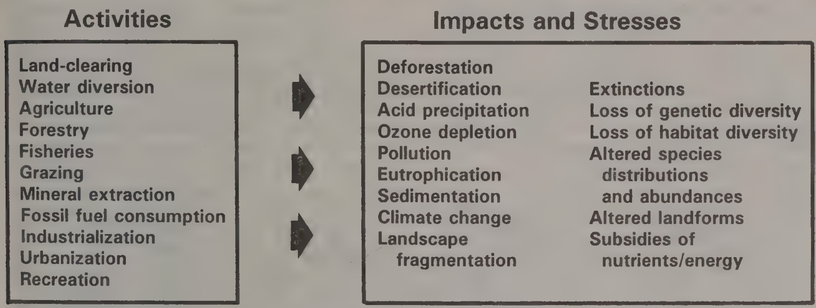

FIG. 4. Human activities affecting sustainability of the biosphere.

that attempt to integrate pattern and process across scales.

- **What are the consequences of environmental variability, including natural and anthropogenic disturbance, for individuals, populations, or communities?** A fundamental concept in ecology states that environmental variation can promote coexistence among genotypes or species. Recent theoretical and empirical results have refined this concept and identified conditions that relate environmental variation to long-term community stability or change. These results have directed attention to the specific ways environmental fluctuations affect populations; the effect of environmental variability upon species interactions; and intra- and interspecific differences in response to environmental variation and to biotic factors such as competition, predation, or mutualism.

III. ECOLOGICAL KNOWLEDGE REQUIRED FOR A SUSTAINABLE BIOSPHERE

Urgent environmental problems and their consequences for human well-being serve as a second starting point (Fig. 3) for identifying ecological research priorities. Human population growth and human activities have profound effects on the environment (Fig. 4); they contribute to global change, loss of biological diversity, and environmental degradation. Many anthropogenic environmental changes have deleterious consequences for human health and well-being. Because the science of ecology is devoted to understanding interactions between organisms and their environments, it is particularly appropriate for ecologists to focus on the complex relationships between humans and the biosphere (National Research Council 1989*c*, Roughgarden et al. 1989, Raven 1990, Edmondson 1991). In this section, we consider some of the ecological knowledge needed to understand and to ameliorate the ecological impacts of human activities.

Among the many environmental problems facing humanity, three are particularly critical, and their solutions require ecological knowledge: global change, the maintenance of biological diversity, and the sustainability of natural and managed systems. These three topics represent different facets of ecological knowledge needed to achieve a sustainable biosphere, yet there is considerable overlap among them. For example, human activities and their ecological consequences alter processes of global change and, at the same time, have immediate local and regional effects on the sustainability of natural and managed systems. Biological diversity is affected by processes occurring at local, regional, and global scales. The SBI recognizes that common ecological processes govern the response of the biosphere to human activities. Therefore, common ecological principles are likely to be involved in the solution of environmental problems.

We discuss large-scale changes in land use, environmental chemistry, and climate in Section A (Ecological Aspects of Global Change), where we focus on interactions between the biosphere and the abiotic realm. In Section B (The Ecology and Conservation of Biological Diversity), we em-

Box 1. Ecological Causes and Consequences of Global Climate Change

The biosphere both regulates and responds to the climate system through physical and chemical feedback mechanisms. An important challenge for ecologists is to understand processes that link species and ecosystems with climate and to predict ecological responses under climates that do not presently exist.

Ecological processes control the release and uptake of many greenhouse gases. Biological systems also exert control over hydrology and surface-energy balances, which are critical determinants of global climate. Albedo, evapotranspiration, soil moisture, and surface roughness are affected by the characteristics of terrestrial and marine biota. For example, certain groups of marine phytoplankton generate sulfate aerosols that act as cloud condensation nuclei. These may increase the extent of high-albedo cloud cover, altering the global radiation balance. Consequently, the composition of phytoplankton assemblages and the physical factors (e.g., upwelling) and biotic factors (e.g., competition or herbivory) that regulate the abundance and distribution of phytoplankton may play an as yet undetermined role in the global climatic system (Keller et al. 1989). Likewise, changes in vegetation canopy characteristics and evapotranspiration may influence regional and even global climate (Shukla et al. 1990). Thus, ecological studies that explicitly link biological and climatic processes will be useful in reducing the uncertainty in global climate models and in predicting the climatic consequences of human activities that alter ecological systems.

Ecological responses to climate change are complex. An important contribution of the paleoecological approach has been to document the relationship between climate change and biological communities of the past. Temperatures predicted for the next century (Jaeger 1988) are higher than any experienced by the Earth's biota during the last several million years, and the projected rates of change may be more than an order of magnitude faster than any global change in the past 2 million years. Major impacts will result from alterations in precipitation and disturbance regimes and in temperature extremes, as well as from changes in mean temperature (Dobson et al. 1989). Geographic shifts in climatic regime may occur faster than some species can disperse to new locations with suitable conditions (Davis 1986, 1989, Graham 1986). Quaternary pollen records show that the compositions of plant communities have continuously changed in response to long-term climatic variations. Changes in animal ranges have occurred for many but not all species, leading to the formation of new species assemblages (Graham 1986). Recent research in population and community ecology suggests that changes in community composition are likely to result not only from direct abiotic limits to species' dispersal, establishment, or persistence, but also from alterations in complex interactions between species and their mutualists, competitors, predators, or pathogens. Prediction of the consequences of climate change will be improved by integrating studies of the current and past distributions of species with mechanistic studies of abiotic and biotic interactions.

Another likely long-term consequence of global climate change is modification of the genetic composition of populations and species. It is difficult to predict the effect of evolved resistance to particular stressors on overall stress-resistance of organisms. Many organisms that can rapidly evolve resistance to environmental toxins (Bishop and Cook 1982) are also likely to evolve rapidly in response to changes in climate and concentrations of greenhouse gases (Holt 1990). Selection for tolerance to heat or desiccation can quickly lead to the evolution of general stress-tolerant genotypes that are resistant to a variety of environmental stressors and that may have altered life history traits (Huey and Kingsolver 1989, Parsons 1989). In contrast, tolerance of certain stresses may increase the sensitivity of organisms to other stresses (Weis and Weis 1989). Ecological studies that predict how climate change might alter population size and migration would contribute to understanding the consequences of global change for genetic variability, genetic drift, and hence evolution, within populations and species.

phasize the processes that affect biological diversity at several scales. Finally, we address local- and regional-scale issues of sustainability in Section C (Strategies for Sustainable Ecological Systems), where we focus on environmental assessment, restoration, and management, including the interface between ecological processes and human populations. In each section, we define the scope of the issues and the significance of ecological knowledge for addressing the problems. In the boxes, we highlight immediate research needs that also suggest the key research topics discussed in Section IV (Research Priorities for a Sustainable Biosphere).

A. Ecological Aspects of Global Change

Human activities are currently leading to unprecedented changes in the Earth's atmospheric, terrestrial, freshwater, and marine environments. Land-clearing, agriculture, fossil fuel consumption, and industrialization add a variety of trace gases and pollutants to the atmosphere. The potential consequences of altered atmospheric composition range from climatic warming and depletion of stratospheric ozone to enhanced biological productivity through CO_2- and nitrogen-enrichment, with subsequent alterations in population, community, ecosystem, and landscape processes. Human activities also divert and deplete surface and groundwater supplies and add toxic substances, wastes, and pollutants to lakes, rivers, and oceans, thus altering the productivity and biological diversity of freshwater and marine ecosystems. Although global change is often equated with greenhouse warming, it is clear that an ecological definition of global change also must include large-scale alterations in patterns of land and water use and anthropogenic changes in environmental chemistry, in addition to climate change.

Changes in the Earth's ecosystems are both a cause and a consequence of altered global environmental conditions. To understand the complex feedbacks that link biota with air and water, ecological research is needed on the role of biotic and abiotic factors in controlling population dynamics, community structure, and biogeochemical cycles. The anthropogenic causes of global change in the hydrosphere, atmosphere, and climate lie in processes occurring at regional scales (e.g., water-diversions, burning of fossil fuels, deforestation, release of chlorofluorocarbons or other pollutants). However, the ecological consequences of global change may be felt first at the individual, population, and community levels. For example, changes may occur in individual organisms (e.g., in altered photosynthetic rates, changed behavior, altered microbial activity) and in community structure, due to altered disturbance regimes and species interactions. Changes in both individual function and community structure may ultimately be expressed as changes in ecosystem function. Thus, biotic and abiotic interactions must be understood across different levels of biological organization and across different spatial and temporal scales.

Three interrelated, immediate needs exist for

Box 2. Direct Ecological Causes and Consequences of Changes in Atmospheric, Soil, Freshwater, and Marine Chemistry

The Earth's biota is both a source and a sink for many trace materials that have potential direct effects on ecological systems. Increasing concentrations of CO_2 can directly influence terrestrial, freshwater, and marine systems. Effects of acid deposition on lake and river systems have been well-documented. Potential effects of nutrient loadings, pesticides, and industrial wastes on ecological processes in soils and estuaries have become an important research issue in recent years. Such effects are destined to become more important as human populations grow.

Studies are needed on the ecological factors controlling material fluxes on land and in freshwater and marine systems, as well as on the feedbacks and consequences of such changes to the functioning of ecological systems. For example, to address questions of the local impact of ozone generated by urban activities in temperate regions and by biomass burning in tropical areas (Andreae et al., *in press*), ecological studies are needed that link the effects of elevated ozone levels at physiological, population, community, and ecosystem scales. To predict the effect of high ozone levels on plant distributions, for instance, an interdisciplinary approach could link physiological studies on the relative sensitivities of plant species to ozone with ecological studies on how differential tolerance alters competitive relationships and susceptibility to herbivores or pathogens.

To address larger scale impacts of regional air, soil, and water pollution on ecological systems, multidisciplinary studies are needed of the effects of human activities on microbial processes, whole-ecosystem biogeochemical cycling, and emission of CO_2 and trace gases to the atmosphere. Ecologists must address a variety of questions in collaboration with scientists from other disciplines to understand the fundamental processes controlling fluxes of materials and their effects on terrestrial, freshwater, and marine systems.

fundamental ecological research concerning global change:

- the ecological causes and consequences of global climate change (Box 1)
- the ecological causes and consequences of changes in atmospheric, soil, freshwater, and marine chemistry (Box 2)
- the impact of land- and water-use change on global and regional processes (Box 3).

Significance of ecological knowledge to understanding global change

Land-use change and other human activities have caused massive changes in the biosphere. Deforestation, soil depletion, contamination of air and water resources, and depletion of biological diversity have resulted in dramatic global change over the last century. The threat of climate change adds a new dimension to existing global problems resulting from human activities. The consequences of human activities directly and indirectly affect and are affected by ecological complexity—the diversity of species and habitats, the patterns of ecological assemblages on the landscape, and differences in the productivity and storage capabilities of ecosystems. Better ecological information will improve predictions of global changes that might result from continued alterations in land and water use and industrial activity. It will also better enable ecologists to predict the long-term consequences of global change for the Earth's resources and populations, providing a basis for better management choices.

Research on the ecological aspects of global change will contribute to basic ecological understanding of processes regulating the Earth's biota. Two fundamental ecological questions lie at the center of this research: What regulates the large-scale dynamics of plant and animal populations? What regulates the fluxes of energy and materials (including nutrients and pollutants) within and between ecosystems? Answering these questions requires ecological studies of fundamental interactions among systems at different levels of biological complexity. New ecological understanding of these interactions will be significantly advanced by more collaborations between ecologists and scientists in other disciplines, including atmospheric science, soil science, oceanography, and environmental toxicology. Answering fundamental ecological questions and extending the scope of ecological knowledge will better enable ecologists to assist decision-makers in devising policies to anticipate, ameliorate, or respond to global change.

Advances in ecological science can contribute to societal decision-making by improving predictions of the global consequences of human activities that alter ecological systems. Ecological studies can elucidate biological processes that regulate ecosystem or climatic processes. The effects of biota on albedo or trace-gas emissions, for example, are not currently well understood. Accordingly, the predictive ability of global climate models would be improved by incorporation of more realistic ecological feedback mechanisms (Schneider 1988).

Ecological advances will also contribute to improved prediction of the responses of the biosphere to the novel conditions expected as a result of global change. Improved understanding of how specific environmental changes affect species and alter species' interactions will better enable ecologists to predict how the distribution of species and communities and the magnitude of productivity will change as a result of natural or human-caused global change.

Theoretical and empirical studies are needed to understand the links among ecological responses at various levels of biological organization. For instance, information gained from physiological studies must be used to couple local and meso-scale models with large-scale climate models. Large-scale and longer term experiments, remote-sensing techniques, and large-scale data sets offer new opportunities for ecologists to synthesize their work at regional and global scales and to cooperate among disciplines.

Most of the needs for research on global change identified in the SBI have been considered in the planning documents of the International Geosphere–Biosphere Program (IGBP) (National Research Council 1988), the U.S. Global Change Research Program (USGCRP) (Earth System Sciences Committee 1988, Committee on Earth Sciences 1990), the Global Ocean Ecosystem Dynamics Research Program (GLOBEC) (1988), the Long-Term Ecological Research Network Office (LTER) (1990), and the Joint Oceanographic Institutions (1990). Some of these issues have also become focal points for research in the IGBP core projects (e.g., tropical land-use change and atmosphere–biosphere interactions; International Global Atmospheric Chemistry Program [IGAC], Glabally 1989). Each of these research plans makes clear the need for strong participation by ecologists in studying the local, regional, and global implications of the changing Earth. However, the relative research effort devoted to ecological and biological questions has been drastically underrepresented in many global change research programs.

Building on these earlier efforts, the SBI calls for increased participation by ecologists in planning and research in on-going programs, especially emphasizing the importance of ecological complexity in global processes and linking studies of global change with efforts to understand biological diversity and the sustainability of local and regional ecological systems.

Box 3. Ecological Consequences of Land- and Water-Use Changes

Over the past century, changes in land and water use have converted natural systems to a variety of managed systems (e.g., agriculture, grazing, urban and industrial uses, or intensive forestry), changing the Earth's atmospheric chemistry and altering the fluxes of materials into freshwater and marine systems. Throughout the world, major diversions of freshwater for agriculture, hydroelectric power, and residential use have severely altered flow regimes and chemistry in major rivers and have destroyed fisheries. The effects of some land- and water-use changes can be detected regionally and globally, and they can alter population, community, and ecosystem processes at substantial distances from the initial change.

Examples of land-use effects on emissions of greenhouse gases are plentiful. Deforestation in the tropics alone has been estimated to contribute a net CO_2 flux to the atmosphere of around 1–2.5 Pg/yr (Detweiller and Hall 1988, Houghton 1990) and may also be a significant cause of the increasing concentration of atmospheric NO_x (Luizao et al. 1989). The use of fertilizers has led to increased fluxes of nitrogen trace gases in temperate ecosystems and may have an even greater impact in the tropics. Growing populations of livestock (Cicerone and Oremland 1988) and increasing rice paddy extent and production may represent substantial sources of the global increase in CH_4. At the same time, water diversions, removal of native vegetation, and conversion to human land uses have increased sediment and nutrient fluxes to surface waters, altering the hydrologic regimes and chemistry of lakes, rivers, estuaries, and near-shore marine systems in much of the world. Consequently, there is an urgent need to understand how the land–water–atmosphere system responds to land-use change.

Multidisciplinary studies at a variety of spatial and temporal scales will be necessary to address the effects of land-conversion and water-diversion on (1) microbial processes in the soil, sediments, and water column; (2) physical characteristics of soils and sediments; and (3) the roles of physiological and ecological processes in the exchange of materials within and among the atmosphere, soils, and water (see Box 2). Such studies will be needed to evaluate the biotic and abiotic consequences of alternative land uses, including the relatively new sustainable approaches and the ever-increasing use of fertilizers.

B. The Ecology and Conservation of Biological Diversity

The diversity of life on Earth constitutes a unique resource for future generations. The 1.4 million species of organisms identified and catalogued to date are only a small fraction of the 5 to 50 million species thought to exist. Human activities have profound consequences for biological diversity at many levels. Habitat destruction is the chief cause of the global extinction rate estimated to be approximately 17 500 species per year, or almost 0.1% of the extant species per year (Wilson 1990). Regionally, species introductions and altered disturbances rates may favor increased local diversity, but habitat loss or modification, outbreaks of introduced or native species, and management of exploitable systems tend to decrease species richness and heterogeneity.

Because only a small fraction of the earth is protected in parks and reserves, and the human population is growing, the accelerated extinction of species and destruction of habitats will continue. Current efforts to conserve biological diversity have focused on diversity at the species level and on prevention of extinction. However, an ecological definition of diversity also must include both the genetic diversity necessary to maintain each species, and the diversity of communities and ecosystems that support them. The goal of preserving diversity at all levels—genes, species, and ecosystems—requires a better understanding of how ecological processes operating on different spatial and temporal scales interact. To resolve the most pressing issues concerning biological diversity, ecologists must

- describe the global distributions of species and their associations and determine the factors that affect rates at which diversity changes (Box 4)
- accelerate research on the biology of rare and declining species (Box 5)
- determine the effects of global and regional change on biological diversity (Box 6).

Animal and plant populations continually face changes in climate, environmental chemistry, water- and land-use patterns, and fragmentation of habitats. Destruction of habitat leads directly to reductions in the size of breeding populations and loss of local genetic variability, both of which increase the likelihood of local extinction. However, these effects may be mitigated if landscape configurations permit the local loss of species or genetic diversity to be offset by immigration from nearby areas. Additionally, water diversion and increased pollution and sedimentation in streams, lakes, and estuaries often leads to degradation of valuable aquatic habitat and the consequent loss of biological diversity. Changes in land-use patterns also cause the natural and semi-natural habitats that harbor most biological diversity to be contiguous with intensely managed agricultural and industrialized urban areas. Although natural areas function as buffers around such managed ecosystems (Goselink et al. 1974), the proximity of natural and managed areas also means that natural populations are necessarily affected by agricultural, industrial, and other urban waste and by demand for resources (e.g., water). Thus, global and regional patterns of human activities need to be linked with descriptions of the abundance and distribution of species and communities and with intensive studies of the ecological processes that regulate diversity.

Synthesis of results from many subdisciplines of ecology will be needed to describe global and regional patterns of biological diversity, to determine the processes that maintain diversity, and to contribute to the conservation of biological diversity at all levels. Ecology has been characterized by numerous approaches and several distinct subdisciplines, such as physiological, evolutionary, community, ecosystems, or landscape ecology. Such variety is healthy and necessary for understanding the processes operating at different spatial and temporal scales that account for patterns of biological diversity. However, particularly challenging collaborative tasks lie ahead; for example, (1) fine-scale individual-based models (i.e., those that emphasize aspects of physiology, behavior, development, and genetics) must be integrated into more coarse-scale ecological models (i.e., those that emphasize population and meta-population structure, species assemblages, community structure, and ecosystem function), and (2) physical aspects of the environment must be incorporated into traditional biologically based studies of populations and species interactions.

Significance of ecology to the conservation of biological diversity

The challenges posed by global change, habitat loss and fragmentation, and species extinctions have the potential to stimulate significant advances in fundamental understanding of ecological processes. For instance, the primary focus of population and community ecology has been to elucidate the manner in which biotic and physical factors interact to account for the distribution and abundance of species. The threat of global change (see Section III A, Ecological Aspects of Global Change) now demands that ecologists extend theoretical and empirical studies in order to predict how populations and species will respond to the anticipated large-scale changes in climate and environmental chemistry. Changes in land and water use and fragmentation of habitat give impetus to studies on the interaction between landscape configuration (including aspects of the size, shape, isolation, and persistence of patches) and patterns of genetic and species diversity. The need to halt the extinction and decline of species directs attention to questions regarding the genetics of small population size; movement, colonization, and invasion dynamics; and the persistence of small populations when interacting with multiple competitors and predators or when establishing new mutualistic relationships. The search for solutions to such problems will stimulate the development of every facet of fundamental ecological science.

Ecologists are increasingly asked to justify the benefits of biological diversity compared to the human benefits that might be derived from economic development. Ecologists will be challenged over the coming decades to evaluate the functional significance of genetic diversity, species diversity, and ecosystem diversity. The ability of ecologists to influence the debate on biological diversity will depend greatly on advances in understanding the functioning of natural systems and the significance of individual species in ecosystem processes. Because human resources are limited, society will weigh the costs of conserving diversity against the long- and short-term costs of its loss. Thus, there is also an urgent need (1) to forge new theory that explicitly incorporates economic as well as ecological principles, and (2) to conduct research on the economics of exploitation and conservation.

Advances in ecological research can contribute to the conservation of biological diversity. Studies of rare and declining species have immediate application in the design of natural areas and the development of management plans for their preservation. Although there is an obvious need to set aside and manage relatively undisturbed areas as preserves, the conservation of the vast majority of species must take place within the "semi-natural matrix" of forests, grazing lands, rivers, and estuaries (Brown 1988). Thus, ecological studies of the effect of land-use change and landscape fragmentation on biological diversity will play an increasingly important role in (1) designing urban and agricultural landscapes that include natural and semi-natural areas, and (2) developing management practices that conserve biological diversity and meet the complex needs of a modern society.

Needs for research on biological diversity have been considered in Congressional and agency initiatives and in various national and international planning documents (e.g., National Research Council 1989*b*, National Science Board 1989, Reid and Miller 1989, Soulé and Kohm 1989, di Castri and Younes 1990, Elsworth 1990, and McNeely et al. 1990). Building on these earlier efforts, the SBI calls for new research programs that focus on (1) the role of biological diversity in controlling eco-

Box 4. Biological Inventory

An ambitious program of biological inventory is needed not only to catalog and map the world's major distributions of species and species associations, but also to link the pattern of distribution of species and habitats with natural and anthropogenic processes that affect biological diversity (Soulé and Kohm 1989). This effort will require coordination among ecologists, systematists, and natural-resource biologists working across very different spatial and temporal scales—from ecosystem ecologists using remote sensing and broad-based landscape analysis to population biologists working on locally endemic, rare forms and genetic varieties. Such investigations will require the establishment of new and perhaps more finely tuned habitat-classification schemes based on multiple aspects of individual species, complex associations of species, and interactions between biotic and abiotic factors. Particular attention must be paid to associations between ecotones and patterns of global and regional biological diversity. An inventory of the world's biological diversity should also incorporate the work of systematists and population geneticists detailing phylogenetic relationships and that of paleoecologists describing the past distribution of species and communities and their responses to environmental change.

Analysis of speciation patterns offers clues to ecological processes that account for changes in biological diversity across broad geographic areas. For example, world-wide distributions often show "centers of endemism," local regions that are particularly rich in endemic species. Approximately 15% of the species in Costa Rica are endemic compared with only 1% in West Germany (Reid and Miller 1989). Remote oceanic islands, such as the Hawaiian and Ascension Islands, show unique constellations of endemic flora and fauna. Patterns of endemism are important to study because endemic species are often rare and subject to higher probabilities of extinction. Accounts of the biological processes that lead to the formation of new species will help in establishing conservation and management programs for rare species.

Box 5. The Biology of Rare and Declining Species

A major focus of conservation biology is the ecological and evolutionary study of rare and declining species.

Rare species. The study of rare species may yield different insights into ecological processes than would studies of more common species. Ecological studies have focused primarily on very common species, but most species are relatively rare. Geographically widespread species may be very uncommon locally. Alternatively, some species may be endemic to a very restricted locale, but may be quite abundant there. The Hawaiian silver sword, *Argyroxyphium macrocephalus,* for instance, is a plant that occurs only in the crater of Haleakala volcano, but is represented there by over 47,000 individuals (Rabinowitz et al. 1986).

Although rare or endemic species are in greater danger of extinction than are widespread, common species, many rare species show prolonged periods of stable persistence. Furthermore, many of today's common species were rare during the past. The ability of species to persist when rare depends on the interaction between species' life history traits and environmental conditions. The life history phenomena that underlie rare species' population growth and, consequently, the likelihood of long-term persistence are thought to be quite different from those of common species. Studies are needed to understand how life history patterns and other traits associated with different forms of rarity interact with environmental factors.

Declining species. The decline of widespread, common species potentially reflects large-scale or long-term environmental changes and is likely to have a large impact on the communities in which they occur. The decline of amphibian species has been associated with local habitat destruction, the introduction of predators, and consumption by humans. However, population declines have also occurred in the absence of these factors, suggesting that other factors such as pesticide pollution, acid rain, low-level increases in ultraviolet exposure, or climate change may be implicated in some cases (Blaustein and Wake 1990). Natural fluctuations may also account for the decline of some species. Because amphibians are major consumers of invertebrates and are eaten by many vertebrates and invertebrates, a decline of amphibians could have ecological consequences that extend throughout many ecosystems. A global inventory (Box 4) is needed to provide the long-term data and the comparisons with other taxa that are required to evaluate the status of declining species.

Evolutionary responses. Long-term evolutionary responses in species that are rare or declining depend upon the underlying genetic structure of constituent populations. Rare or declining species may often exist only as small, locally isolated populations that are subject to increased inbreeding. The determination of breeding structure, effective population size, and inter-population movement is essential for understanding the potential for persistence or recovery of such populations.

Strategies for preserving endangered species also will require information on the genetic and demographic constraints to adaptation in individual species. Evolutionary change depends on the pattern of variance in important traits and the covariance among essential traits, as well as on the rate of environmental change and the population size and age structure. New developments in the genetic theory of life history phenomena will be important in understanding whether populations can adapt to environmental change.

Colonization. Conservation programs may ultimately rely upon introducing endangered species into new habitats, necessitating increased research on the dynamics of colonization and invasion. What features of a species enable it to succeed as an invader or as a colonist? How does success as an invader depend on the network of interactions with species already present in the community? What conditions promote the establishment of early colonists? Ecological studies of the processes and factors that regulate both the number of species in a community and the dynamics of species replacements will help provide answers to these questions.

logical processes, and (2) the complex suite of ecological processes that shape patterns of diversity. Such ecological studies would also contribute to understanding the processes underlying global change and the principles necessary for sustainable use of the biosphere.

C. Strategies for Sustainable Ecological Systems

Humans depend on natural and managed ecological systems for food, shelter, clothing, and clean air and water. As demands for the goods and services of the biosphere increase, so does the need to understand the complex array of interactions between humans and the biosphere. Ecological approaches to understanding environmental change increasingly will include the roles of humans both as agents of change and as populations responding to change.

Virtually every ecosystem on Earth has been influenced, to some extent, by the activities of humans. Effects range from the indirect influences of globally distributed pollutants on remote, uninhabited areas to the direct influence of activities that remove species, alter their distributions, or restructure entire landscapes. In addition, large areas of the Earth's surface are covered by ecosystems, such as agroecosystems and forest plantations, that have been designed and maintained by humans. Hallmarks of these managed systems are low species diversity; the infusion of large quantities of energy and nutrients to maintain them; and the extraction of additional energy, biomass, and nutrients. Many ecosystems are also used for recreation, for watershed management, or as reserves to maintain biological diversity.

As the human population continues to grow, it will place additional heavy demands on the earth's ecosystems. Even if the world's population equilibrated today, the pressure to increase the quality of the lives of existing people would tax the Earth's resources. To prevent or reverse the degradation of the resources of the biosphere, human use of those resources must be made sustainable. Advances in the political, social, and economic spheres, in agronomy and resource management, as well as in ecology are needed to work toward the goal of sustaining the biosphere (Brown 1989). The current generation of humans must accept the challenge to develop methods for deriving needed resources from the environment, and for making use of it in other ways, without compromising the ability of future generations to maintain themselves and to sustain their quality of life.

Box 6. Effects of Global and Regional Change on Biological Diversity

Ecologists are now being asked to predict the impact of climate change and changing land-use patterns on biological diversity (Soulé and Kohm 1989). How do changes in environmental chemistry, global temperature, patterns of precipitation and wind stress, or oceanic circulation affect population dynamics and global species diversity? What are the implications of increasing fragmentation of once large and continuous habitats? Ironically, most ecological models of population growth and species interactions focus almost solely on biotic rather than on physical factors such as temperature, precipitation, atmospheric or aquatic turbulence, or landscape configuration.

A renewed focus on the role of abiotic forces in structuring biotic assemblages is in order. Although nutrient concentrations and ratios have been included in models of both terrestrial and aquatic plant communities, these models often do not incorporate other factors such as solar radiation, temperature, and soil moisture. However, models of crop and forest production explicitly consider the influence of daily temperature and precipitation patterns on crop growth. These models could be used to predict the impact of climate change on short-term plant growth by incorporating the temporal patterns of temperature and precipitation derived from climate models. One of the great challenges will be to integrate similar fine-scale models with community and ecosystem models (e.g., forest-gap models) to predict the long-term consequences of climate change on biological diversity (Huston et al. 1988).

Human activities turn natural landscapes into mosaics of croplands, forests, and abandoned areas in different stages of succession. Many animal and plant species occupy a range of different habitat types in these complex landscapes and may exhibit different demographic characteristics in different habitat types. Greater attention must be paid to habitat-specific demography and life history phenomena as well as local adaptive changes in reproductive biology. We need to better understand the effects of landscape pattern (i.e., the sizes, shapes, and arrangement of habitat patches) on population size, dispersal, and diversity at the local landscape level. When suitable habitat is fragmented, the intervening habitat may impede dispersal to varying degrees. Therefore, the matrix between habitat patches, as well as the distance between the patches, may greatly influence the regional stability of populations, establishment of new populations, and long-term persistence of mobile species. Such matrix-dependent processes require ecologists to focus more attention on how the specific geometry of landscapes influences biological diversity (articles in Burgess and Sharpe 1981, Turner 1987).

Box 7. Indicators of Ecological Responses to Stress

Human activities induce stress in ecological systems by introducing pollutants, by altering landforms, and by directly adding or removing organisms. These activities indirectly affect species composition and alter interspecific interactions within the affected communities, ultimately changing the flux of natural and anthropogenic materials through the system (Levin et al. 1989). To understand and ameliorate the effects of anthropogenic stresses on natural systems, research is needed on how different stresses affect the behavior and physiology of individuals, population and community processes, and ecosystem function within particular systems and among systems (Westman 1985). In addition, the potential for interactions among multiple stressors requires further explication (e.g., Sheehan et al. 1984). Required research includes detecting and quantifying patterns in space and time and explicating underlying mechanisms.

Indicators. A major empirical problem is the definition and measurement of ecological responses to various stresses. The lack of sensitive indicators of environmental stress limits detection of the early stages of ecological change, and this seriously impedes understanding and effective management of ecological systems (Barrett and Rosenberg 1981). In some ecosystems, functional measurements of ecosystem processes (such as productivity and nutrient cycling) are often less sensitive indicators of ecosystem stress than are structural properties such as species composition (Schindler 1987). Sometimes extensive degradation has already occurred by the time ecosystem-level functions change. Thus, individual populations or attributes of communities are likely to be better indicators of ecosystem response to stress (Karr 1991).

Ideally, indicators would be chosen on the basis of the speed of their response or their sensitivity to specific stresses (Cairns 1977, National Research Council 1986). Because unperturbed populations, communities, and ecosystems may be quite variable through time, it is essential to know the baseline variability of the physical environment and of the selected biological indicators in order to determine whether undesirable change has occurred (Sheehan et al. 1984). It remains to be seen whether indicators that optimize the ratio of sensitivity to variability can be developed.

A great deal of basic research is needed before indicators of environmental change can be used with confidence. The development and testing of environmental indicators requires (1) long-term studies to establish baseline variability; (2) field perturbation experiments of appropriate spatial scale, intensity, and duration to test the sensitivity and specificity of indicators (Likens 1985, Schindler 1987); and (3) comparisons of systems exposed to stresses of different types and magnitudes (Steele et al. 1989, Cole et al. 1990). Access to long-term research sites and data bases (Strayer et al. 1986, Likens 1987), which may be shared by many projects (Kitchell et al. 1988), offers ecologists opportunities to develop and test ecological indicators in interdisciplinary settings.

Test systems. To assess the environmental consequences of particular human activities, test systems and rules for extrapolating from test systems to natural or managed systems must be developed. The problem of extrapolation is central to the development of test systems (Levin et al. 1989), involving basic principles of scale in ecology. The spatial scale or organizational complexity of an ecological system, and the type, duration, and frequency of anthropogenic stresses may affect the response of the system to a particular stress. Verification of rules for extrapolation requires experimental and observational tests at a number of scales (Frost et al. 1988), involving collaboration among scientists and agencies (Mooney et al. 1991). Thus, ecological research is needed at scales commensurate with restoration and management of entire natural systems.

To promote a sustainable biosphere, ecological science must

- determine patterns and indicators of the responses of ecological systems to stress (Box 7)
- provide guidelines and techniques for the restoration of ecological systems (Box 8)
- develop and apply ecological theory to the management of ecological systems (Box 9)
- further develop our ecological understanding of introduced species, pests, and pathogens (Box 10), and apply ecological theory to the management of infectious diseases (Box 11)
- develop interdisciplinary and multi-disciplinary approaches that integrate ecology, economics, and other social sciences (Box 12).

Although the exact meaning of "sustainability" is actively debated (Shearman 1990), we use the term to imply management practices that will not degrade the exploited systems or any adjacent systems (Turner 1988). Achievement of sustainability often requires both minimal subsidization of managed systems so they are relatively self-sufficient, and restoration of damaged systems whose goods and services are essential to human well-being. Because unchecked growth of the human population and misuse of natural resources degrades the biosphere, sustainability also implies "consumption standards that are within the bounds of ecological possibility and to which all can aspire" (World Commission on Environment and Development 1989).

Natural systems provide a point of reference for defining and detecting environmental degradation (Box 7) and creating models for environmental restoration and management (Boxes 8 and 9). In addition, significant interactions link managed and natural systems at many scales. For instance, managed systems are often critically affected by "wild" species. They may be pests or pathogens that reduce productivity (Box 10), or they may play a beneficial role, serving as sources for recruitment in restoration projects, as essential symbionts of harvestable species (e.g., as pollinators or mycorrhizae), or as agents of biological control (e.g., as predators, pathogens, or competitors of pest species). Using knowledge gained from natural systems to generalize about processes in managed systems depends on ecological research that explicitly compares processes in natural and managed systems and that focuses on interactions at their interface.

Box 8. Restoring Ecological Systems

Restoration has been called the "acid test for ecology" (Bradshaw 1987) and the "ultimate test for ecological theory" (Ewel 1987). Numerous attempts have been made, with varying degrees of success, to restore degraded ecological systems (Holdgate and Woodman 1986, Ashby 1987, Kline and Howell 1987). Improving the success rate and cost-effectiveness of restorations requires a better understanding of such fundamental ecological processes as nutrient cycling, succession, competition, and predation, and of the interaction of biotic and abiotic factors.

Effects of abiotic factors on the biota have a long and distinguished history in ecological research. Many of the problems associated with restoration involve a poor understanding of how physical factors in degraded systems limit the establishment and growth of species. Physical factors may affect recovering populations directly. For example, attempts to restore mining spoil sites have been retarded because, following initial preparation, the soil collapses to a dense medium through which roots cannot easily penetrate (Rimmer 1982). Physical factors may also affect species indirectly through their effects on interspecific interactions. Exposure to stress, for example, may alter the susceptibility of recovering plant populations to herbivory (Louda 1988).

In addition, population and community processes may have potent effects on ecosystem processes. Fluctuations in certain populations may reverberate throughout all trophic levels, causing changes in productivity, nutrient cycling, and fluxes of contaminants and pollutants. It is now apparent that the biogeochemical heterogeneity of continents has been structured significantly by animal population dynamics (Naiman 1988). Ecologists also recognize that animal population dynamics are coupled at continental and intercontinental scales (Brown and Maurer 1989, Holling 1988).

Basic research on the couplings between community processes and ecosystem functions is fundamental to progress on ecosystem restoration. Ecological research can provide a conceptual framework to guide ecological restoration projects and increase their effectiveness. To facilitate the development of such a framework, financial and institutional support is needed for research on a broad scope of community and habitat types and on all ecological aspects of restoration, from population genetics to ecosystem function.

Significance of ecological science to the development of sustainable ecological systems

Research aimed at developing ecological strategies for a sustainable biosphere will advance fundamental understanding of ecological processes. As in the past, applied ecological studies in human-affected systems will continue to make significant contributions to understanding basic ecological phenomena (e.g., population dynamics,

Box 9. Developing and Applying Ecological Theory to the Management of Ecological Systems

Managed and natural ecosystems form a continuum from monocultures of row crops to pristine, unexplored sites. Intermediate degrees of management are applied to semi-natural systems such as fisheries, grazing and forest lands, and national parks. Managed systems generally have lower genetic and species diversity than natural systems, with genotypes or species adapted to relatively constant environmental regimes. Relatively open nutrient cycles in managed systems often result in significant impacts on surrounding systems. Managed systems are usually subjected to frequent, severe, intentional perturbations (i.e., management) that interfere with long-term ecological processes.

Because human well-being depends upon ecological systems, managed systems must be characterized by stability or by resiliency as environmental change occurs. Lessons from natural systems suggest that the sustainability of managed systems may be enhanced by closed nutrient cycles (Coleman and Hendrix 1988), increased species and genetic diversity, and decreased negative influences on surrounding areas (Cox 1984). In a sense, "designer" ecosystems must be constructed with natural ecosystems serving as the model (see Coleman 1989).

Experiments. The science of ecology has much to contribute to ensure the sustainbility of ecological systems in the face of human exploitation. In addition, the advance of ecological science will greatly accelerate if management actions can be structured as large-scale experiments. Large experimental perturbations have a distinguished history of contributions to ecosystem ecology (Likens 1985), and are essential for rapid evaluation and comparison of alternative management strategies (Walters 1986). Every major development project or management intervention is a learning opportunity if adequate baseline and follow-up data are collected, and a proper statistical approach is employed. By linking such large experiments with studies in smaller scale test systems, non-linear effects, interactions among factors, and the roles of covariates can be understood. Collaborations among ecologists, statisticians, and managers offer the prospect of developing powerful new experimental tools for evaluating the consequences and effectiveness of management options (Matson and Carpenter 1990).

Modeling. Ecological modeling is undergoing rapid advances and improvement. A new generation of models will incorporate the effects of physicochemical factors and community-level interactions to analyze the dynamics of managed populations or ecosystems. Management experiments offer the opportunity to develop, test, and improve models at scales ranging from individual organisms to ecosystems (Kitchell 1991). Strong manipulations at the scale of management force "informative failures" of management models and lead to rapid identification of the models that perform best in a management context (Walters 1986).

Today, the discipline of ecology faces the challenges of enlarging ecological perspectives to include human values and needs and to identify the major ways in which managed and natural ecosystems affect each other's long-term well-being. If managed ecosystems are viewed as integrating a local community with farm, non-farm, and natural resource (forest, wetland, aquatic) sectors, then research is needed to examine interactions beyond the farm, forest, or park gate and the impact of social and economic forces (National Research Council 1990). A world so altered by human activity offers the opportunity and the challenge to expand the scope of the discipline of ecology.

succession, predator–prey systems, and ecosystem processes). Increasingly, the need for extrapolation and generalization of ecological principles at scales similar to those of environmental assessment, restoration, and management will promote the development of theoretical and empirical approaches that link processes across scales. In addition, ecologists will be challenged to integrate human-induced perturbations (with their characteristic type, frequency, duration, intensity, and extent) into models of the effects of stress and disturbance on populations, species interactions, and ecosystem processes.

Ecological science can provide some of the tools needed to assess, restore, and manage Earth's life support systems. To define and detect environmental degradation, and to guide the restoration of ecological systems, studies are needed to link population- and community-level processes with ecosystem function. In addition, ecological studies are necessary to elucidate the role of biota in mediating the transport, fate, and effects of pol-

THE SUSTAINABLE BIOSPHERE INITIATIVE
RESEARCH PRIORITIES

- ♦ **Global Change**
- ♦ **Biological Diversity**
- ♦ **Sustainable Ecological Systems**

FIG. 5. Research priorities: understanding the role of ecological complexity in global processes, the ecological causes and consequences of biological diversity, and the underlying ecological processes that affect the sustainability of natural and managed ecological systems.

lutants and toxicants in the environment. Ecological approaches to sampling, statistical analysis, and experimental evaluation of underlying mechanisms will be useful in improving tests of the environmental consequences of restoration and management strategies.

In all of this work, it will be essential to combine studies of human populations with those

Box 10. Introduced Species, Pests, and Pathogens

The importance of introduced species, pests, and pathogens cannot be overlooked, whether we are restoring ecosystems, creating new ones, or trying to predict changes in existing systems. Many anthropogenically altered ecosystems have been characterized by problems associated with pests. In the future, as species are introduced or move in response to environmental changes, some of today's desirable species may become pests in their new environmental contexts, while some pests may become more pernicious.

Control of agricultural pests can depend on cultural practices (Phillips et al. 1980), the introduction of biological control agents (Batra 1982), the use of chemical pesticides, or combinations of these and other methods. Building on a long history of study of pest-control techniques, ecological research is needed to improve understanding of the biological basis of control. For example, research is needed to resolve controversies over the nature of predator–prey population dynamics (Hassell et al. 1989) in successful and unsuccessful biological control (e.g., Murdoch et al. 1985, 1989); the number of species of natural enemies and biological attributes of such species to be used in biological control programs (e.g., Crawley 1987, Myers 1987, Myers et al. 1989); and the source area for introduced natural enemies and the degree of their prior evolutionary exposure to the pest (Hokkanen and Pimentel 1989, Pimentel and Hokkanen 1989). Additionally, the degree of synergistic or antagonistic interactions of pests (Allen and Bath 1980, Haynes et al. 1980) under changing scenarios (Pimentel 1977) requires further explication.

Introduced species and genetically altered organisms are potential "pests" that deserve ecological consideration. "Designer" ecosystems may include introduced or altered species (Whalen 1986, Gasser and Fraley 1989). Will any of these forms "escape" and become pests (Ellstrand and Hoffman 1990)? What is their potential if introduced into relatively unmanaged systems (Doebley 1990)? Critical ecological experiments are needed to test specific hypotheses posed by these questions (e.g., Regal 1987, Regal et al. 1989, Tiedje et al. 1989, Hoffman 1990).

The spread of infectious diseases is an ecological phenomenon—essentially a host–parasite interaction. This point is often ignored in epidemiological studies, although the earliest epidemiological models (e.g., those for malaria) were explicitly ecological. More recently, viral and other diseases have been examined within the same framework that has been used for epizootics (Anderson and May 1979, May and Anderson 1979). Melding techniques from epidemiology and ecology, this approach considers disease-induced mortality and variable population size, non-homogeneous mixing, and other ecological factors. Evolutionary considerations, such as the evolution of reduced or increased virulence, also provide a wealth of research questions and may suggest possible ecological approaches to disease management.

that examine changing patterns of resource use, air and water quality, or global and regional climates. Some of the most important research topics of the coming decade will be at the interface of the social, economic, and ecological sciences. These topics include both the effects of humans on the environment and the consequences of environmental change for human populations and human well-being.

The task of assessing, restoring, and managing sustainable ecological systems can only be addressed by a comprehensive, organized research effort. Current efforts to assess and restore specific ecosystems (e.g., wetlands, mining sites) or to manage sustainable systems (e.g., agricultural, forest, or fisheries resources) represent initial, necessary steps toward the goal of sustaining the biosphere. However, these efforts are not presently united in a comprehensive research framework. Such a framework is needed because ecological processes link natural and managed populations to ecosystems and because common ecological principles underlie effective management strategies. A comprehensive approach is also needed to link studies of sustainable management practices to issues of global change and biological diversity.

The foundations of a more comprehensive approach to research on sustainable ecological systems have been laid by scientists working in the fields of conservation biology (e.g., Soulé and Kohm 1989, Raven 1990) and sustainable resource use (National Research Council 1989*a,* 1990). The SBI proposes the formulation of an integrated research framework to coordinate existing research efforts and to initiate new research programs devoted to enhancing the sustainability of the biosphere.

IV. RESEARCH FOR A SUSTAINABLE BIOSPHERE: PRIORITIES AND KEY TOPICS

After considering intellectual frontiers in ecology and the ecological knowledge required to help solve urgent environmental problems (Fig. 3), we have identified three research priorities (Fig. 5)—**global change**, **biological diversity**, and **the sustainability of ecological systems**. These three priority areas are developed in Section III, where we define their scope, discuss their significance, and identify research needs. In the present section, we introduce key research topics that ad-

Box 11. The Ecology of Disease Spread

Recent efforts to refine ecologically based models of disease transmission are yielding new insights that will improve efforts to control human disease (Anderson 1989). These models, and related empirical studies, have identified several factors that have complex consequences for the transmission of disease in human populations (Hassell and May 1989).

The frequency and nature of contact between infected and susceptible individuals largely determines the spread of disease. Rural–urban migration, for instance, affects the probability of contact by changing both the movement patterns of individuals and population densities. Patterns of behavior may also influence the rate of spread of certain diseases (e.g., the number of partners in sexually transmitted disease). Interdisciplinary studies linking ecology, human demography, and social sciences can contribute to a better understanding of the role of migration and behavior in disease transmission.

Disease transmission often involves multiple hosts with complex life history patterns, as in schistosomiasis. Ecological life history analysis coupled with modern techniques for sensitivity analysis can identify sensitive links in the transmission cycle, resulting in better programs for eradication and control.

Disease transmission may also be regulated by intrinsic and extrinsic factors affecting the susceptibility of individuals. Susceptibility to infectious disease may vary with age, gender, race, genotype, or other intrinsic traits of individuals. Furthermore, extrinsic factors, such as malnutrition, exposure to toxic chemicals, or migration-induced stress may alter susceptibility to disease. By taking such ecological factors into account, the reliability of epidemiological models as tools for the management of public health will continue to improve (Anderson 1989). Additionally, the ecological perspective—with its emphasis on population and evolutionary processes—increasingly will be integrated into immunological, human genetic, and environmental health perspectives on the spread of human disease.

dress the three priority areas and show the links among them. Research in each of these areas has the potential to advance the discipline of ecology and to produce essential information for solving environmental problems.

The three priority areas are interrelated. Because elements of the biosphere are naturally linked by ecological processes, a given human activity may have implications for all three areas. For example, deforestation may alter regional climates by affecting the hydrological cycle, may reduce local species diversity by removing habitats and inhibiting dispersal, and may threaten the sustainability of fisheries by increasing sedimentation in streams within the watershed.

Moreover, the three priorities pose common challenges to the discipline of ecology. For example, implementation of each of these priorities

Box 12. Ecological Processes and Human Populations

No discussion of the Earth's environmental problem is complete without explicit consideration of the growth and shifting demographic patterns of the human population. As the world's population continues to expand, and as developing nations move toward standards of living that imitate those of the more developed nations, the effects of human population growth on the Earth's resources will accelerate. It is essential to consider the impact of increased economic demands for renewable and nonrenewable resources on ecological systems, and to recognize that humans are essential elements of the ecosystem we study.

The issues associated with population growth are broad, involving such factors as changes in per capita income and resource distribution; increasing pollution and environmental degradation; problems of health and poverty; the effects of urban, industrial, and agricultural expansion; and especially the integration of ecological and socioeconomic considerations. Even those factors that are primarily economic will have substantial environmental effects.

The human population now numbers 5.2 billion, and is increasing at the rate of approximately 1.8%/yr. The average growth rate, however, masks disparities among populations of different regions and different nations. The change in demography from a situation of high birth rates and high death rates to one of low turnover, termed "demographic transition," has occurred in most of the developed world but has not occurred in most of the developing world. In much of the developing world, death rates greatly declined after World War II; however, birth rates in many cases increased and have only recently begun to decline. The ecological implications of demographic transition in a large number of developing nations have not been explored fully.

Today, many developed countries have replacement total fertility rates (TFR) of about 2.1, corresponding to the average number of surviving children a woman will have in her lifetime. Such a replacement pattern generates a stable population size. However, many developing countries have TFR's of 4 or more, implying rapid population growth. Efforts to reduce birth rates will require more information and expertise on interactions among human populations and resources. The social and economic constraints that prevent the appropriate and effective use of resources must also be understood.

The effects of human population growth on human health and welfare cannot be treated independently from issues of resource distribution and availability. Often increasing levels of poverty and disease in specific geographic locations can be attributed more to shifting patterns of agricultural production than to strict increases in population size. For example, in some regions of Central America shifts from domestic to export crop production contribute more to poverty and malnutrition than does increasing population growth (Durham 1979). An ecological analysis of human demographic patterns must incorporate the long-term effects of shifting patterns of resource availability and distribution along with the socioeconomic implications of these changing patterns.

There is a real need to bring ecological techniques, especially methods from population biology, to bear on the problems of human population growth. Such studies would require detailed investigations of human demographic structure, variation in growth patterns across different regions, implications of migrational patterns, and shifting age structure. These investigations must be related to studies on changing patterns of energy use, resource production and distribution, disease spread, and urban-industrial expansion. To fully understand how human populations affect and are affected by ecological processes, the complex interfaces between ecology and social and economic sciences and policy analyses must be developed to a much greater extent.

TABLE 1. Key research topics that cut across the three priority areas.*

Key Research Topics
Examples of Research Questions

1. Determine the ecological causes and consequences of global climate change by quantifying and modeling the links between biospheric and global change.
   - What are the differences among biomes and among species within biomes in regulating interactions between the biosphere and the abiotic realm (i.e., the atmosphere, hydrosphere, and lithosphere)? How does community composition affect ecosystem function?
   - How do canopy- and ecosystem-scale energy, water, and gas exchange processes interact with the physical climate system?
   - How do the direct and indirect effects of changing physical and chemical environments alter ecological communities and the population dynamics of component species?
   - To what extent are species' ranges determined by the direct effects of climate or other physical factors, as opposed to biological interactions?
   - How would changes in rainfall distribution affect food supply? How would this affect human population dynamics?
   - How does climate change affect plant and animal dispersal and colonizing abilities?
   - To what degree does the paleoecological record permit prediction of future ecological and evolutionary responses to global change?

2. Determine the ecological causes and consequences of changes in atmospheric, soil, freshwater, or marine chemistry, using fundamental models of how ecological systems regulate the chemistry of the biosphere and models for the ecological consequences of changes in these processes.
   - What are the consequences of increasing CO_2 for biotic interactions in terrestrial ecosystems?
   - What are the relative sensitivities of animal and plant species to regional air pollution?
   - Is the ocean an effective buffer for increased atmospheric CO_2 inputs and, if so, what are the consequences of enhanced oceanic productivity?
   - How do elevated levels of nutrients affect plant–herbivore interactions? How are those changes transmitted through higher trophic levels?
   - How are community composition and species diversity affected by persistent toxic substances?
   - How does chronic exposure to pollutants affect human susceptibility to disease? What are the consequences for rates or patterns of disease transmission?

3. Determine the ecological consequences of land- and water-use change through a functional understanding of how land conversion and water diversion affect ecological processes.
   - How do individuals, populations, and ecosystems respond to the scale, frequency, pattern, and type of disturbance?
   - How do the alterations in species composition that accompany land-use changes affect nitrogen and carbon trace gas emissions to the atmosphere?
   - How do land-use changes and water diversions affect river-basin and other water-body processes and terrestrial–aquatic interactions?
   - What are the relationships between land-use patterns and various measures of water quality?
   - What is the effect of landscape fragmentation on local and regional patterns of diversity?
   - How do land-use change and land conversion affect biogeochemical processes and trace-gas emissions?
   - How does land-use change affect human population structure?
   - What roles do wetlands of various types play in the production of wildlife and fisheries?

4. Determine the evolutionary consequences of anthropogenic and other environmental changes.
   - Under what conditions should new genotypes evolve in response to environmental changes, including climate changes and new sets of species interactions?
   - How does the relative likelihood of evolutionary response vs. extinction change with the rate of climate change?
   - How are demographic parameters of species and interspecific interactions affected by evolutionary changes in physiological tolerance?
   - What are the evolutionary consequences of stage- or age-specific toxicity effects?

ABLE 1. Continued.

- What are the ecological and evolutionary consequences of long-term, intense exploitation of natural populations?

5. Inventory the patterns of genetic, species, habitat, and ecosystem diversity. Determine the rates of change of biological diversity and the subsequent effects on community structure and ecosystem processes. Accelerate research on factors determining diversity at all levels.
    - What are the distributions in the world of species and community types?
    - What are the rates of loss of biological diversity across different habitats and taxonomic groups?
    - What are the key species whose presence or absence can critically alter the composition of local communities?
    - What processes account for the patterns in biological diversity across broad geographic ranges? Do speciation patterns serve as clues to those processes?
    - How are life history traits, reproductive success, evolution, and genetics coupled through reciprocal constraints?

6. Accelerate research on the biology of rare and declining species and develop the scientific information necessary to sustain populations of potentially valuable rare and declining species.
    - What are the evolutionary responses of rare species to environmental change and to long-term conservation strategies?
    - What factors control the dynamics of colonization and invasion by recovering populations?
    - How do the reproductive biology and behavior of individuals of rare species respond to stress?
    - How does genetic structure affect the long-term evolutionary responses of populations that are becoming rare?
    - What role do ecological processes play in the social, political, and economic trade-offs of different conservation or management strategies?
    - What common features distinguish species that have persisted over long periods in the past?

7. Determine patterns and indicators of ecological responses to stress, leading to technologies necessary to assess the status of ecological systems, to forecast and assess stress, and to monitor the recovery of damaged ecological systems.
    - What are early indicators of stress, and what is the ecological significance of changes in such indicators?
    - Can model systems be designed to adequately test the consequences of proposed human activities?
    - What are the empirical scaling rules for extrapolating from model to natural systems?

8. Accelerate the basic science of restoring damaged and degraded ecological systems, by developing, testing, and applying principles of restoration ecology.
    - How is the structure within biological communities (e.g., genetic structure, composition, or species diversity) linked with the functional aspects of ecosystems (e.g., productivity, nutrient cycling, or sequestration and release of contaminants)?
    - How can ecological and evolutionary principles provide a framework to guide restoration projects?
    - What are the separate and combined effects of physical and biotic factors in limiting the establishment and growth of recovering species in degraded systems?
    - How do species' life history traits affect population and community structure?
    - What are the economic and social trade-offs for different restoration options?
    - Under what conditions is mitigation an ecologically defensible policy?

9. Advance, test, and apply ecological principles for the design and use of sustainable, managed ecological systems at appropriately large scales.
    - How do physical factors and community-level interactions affect the productivity of populations of exploited species?
    - Is there a "minimum mix" of species, guilds, and life forms that would result in sustainability of a particular system?
    - Will native animals and microbes persist and participate in sustainable ecosystems composed of novel combinations of plant species?
    - What are the mechanisms allowing or preventing the coexistence of species?

TABLE 1. Continued.

10. Determine the principles that govern outbreaks and patterns of spread of pest and disease organisms.
    - What are the effects of climate change scenarios on the redistribution of pests (including human disease vectors), potential pests, and their host organisms?
    - Why do pest populations vary in their abundance, environmental impact, and susceptibility to extinction?
    - Are multiple-predator, multiple-parasite combinations more effective than single agents in the control of target species?
    - Will parasite and predator species switch to different host species when the population of the target species becomes so low that a residual population cannot be maintained?
    - How do specific environmental changes (e.g., deforestation, drought) alter transmission of infectious diseases in human populations?

* The key research topics listed in this table are derived from the research needs discussed in the various boxes in Section III. Research on each topic may address needs discussed in several boxes.

will require a better understanding of the interactions between the biotic and abiotic components of ecological systems; better integration of population biology with ecosystem science; better synthesis of ecological with evolutionary approaches; and new theoretical and empirical studies that relate patterns across disparate spatial, temporal, and organizational scales.

Recognizing the interrelatedness and common ecological foundations of the three priority areas, we have identified 10 key research topics (Table 1) that further define the three priority areas. The order of presentation of the research topics does not reflect differences in their importance. Instead, each topic represents an integral part of the SBI—fundamental research needed to help solve environmental problems. For each research topic, we have listed examples of the types of research questions that might be addressed. These lists are not intended to be exhaustive, but to suggest the range of ecological research approaches required to address each research topic.

V. RESEARCH RECOMMENDATIONS

Three specific research recommendations emerge from unmet research needs in the three priority areas of the SBI (Fig. 3).

RESEARCH RECOMMENDATION #1: Greater attention should be devoted to examining the ways that ecological complexity controls global processes.

Within the topic of global change, insufficient attention has been paid to the ways in which ecological complexity controls global processes. Such key factors as species and habitat diversity, patterns of distribution of ecological assemblages, and differences in the productivity and storage capabilities of different types of ecosystems all influence how the biosphere functions in the Earth system. The role of this ecological complexity must be incorporated if we are to understand global processes.

RESEARCH RECOMMENDATION #2: New research efforts should address both the importance of biological diversity in controlling ecological processes and the role that ecological processes play in shaping patterns of diversity at different scales of time and space.

Within the topic of biological diversity, much of the current effort is devoted to enumerating the species in various habitats and to preserving biotically significant sites. These important efforts lay the groundwork for the research proposed here and must be continued, but two vitally important topics must also be understood. First, it will be necessary to discover to what extent patterns of biological diversity are important in determining the behavior of ecological systems (e.g., responses to climate change, rates of nutrient flows, or responses to pollutants). Only when these relationships are known will it be possible to develop management strategies for maintaining natural and human-dominated ecological systems. Second, it will be necessary to document how ecological processes interact with physical and chemical factors to control or determine biological diversity. Doing so will require investigation of the manner in which individual species interact with and are modified by the abiotic environment on both ecological and evolutionary time scales.

RESEARCH RECOMMENDATION #3: A major new integrated program of research on the sustainability of ecological systems should be established. This program would focus on understanding the underlying ecological processes in natural and human-dominated ecosystems in order to prescribe restoration and management strategies that would enhance the sustainability of the Earth's ecological systems.

Plans for comprehensive programs in the areas of global change and biological diversity are more advanced than those in the area of sustainable ecological systems. Research programs exist to develop specific sustainable natural resources. However, current research efforts are inadequate for dealing with sustainable systems that involve multiple resources, multiple ecosystems, and large spatial scales. Moreover, much of the current research focuses on commodity-based managed systems, with little attention paid to the sustainability of natural ecosystems whose goods and services currently lack a market value. Addressing the topic of sustainable ecological systems will require integration of social, physical, and biological sciences.

These Research Recommendations are made to ecologists, to researchers in related disciplines, and to funding agencies whose interests encompass one or all of the research priority areas. Immediate and long-term research programs and funding for research in these areas is vital to the success of the SBI.

VI. IMPLEMENTATION: AN ACTION PLAN FOR THE ECOLOGICAL SOCIETY OF AMERICA

The Sustainable Biosphere Initiative identifies the research needed to provide the ecological knowledge required for a sustainable biosphere. Successful implementation of the SBI will require a significant increase in research in the three priority areas. Successful implementation will also require interdisciplinary interactions that link ecologists with the broad scientific community, with mass media and educational organizations, and with decision-makers in all sectors of society (Fig. 1). Obtaining the ecological knowledge needed for a sustainable biosphere necessitates interdisciplinary projects involving collaboration between ecologists and scientists in the natural and social sciences. In addition, achieving a sustainable biosphere will require dissemination and application of ecological knowledge.

Achieving the goals of the SBI will require separate and coordinated activities by scientists and administrators in academia, in government agencies and private organizations, and in business and industry. In this section, we identify specific activities planned by the Ecological Society of America to address the research recommendations and to further develop the educational and environmental decision-making components of the SBI. We also consider the international dimensions of the SBI and the funding needed to implement it. In addition to these activities planned by the Ecological Society of America, implementation of the SBI will require complementary actions by individuals and institutions (Fig. 6). Individual principal investigators, program managers within Federal agencies, policymakers in governmental and non-governmental organizations, and private foundations hopefully will identify special opportunities within their purview to address the objectives of the SBI.

Research

It is crucial that modern science preserve a pluralistic approach to solving scientific problems. The research opportunities described herein demand new combinations of scientific disciplines and the application and expansion of recently developed research tools. To address these research priorities most effectively, it is important to draw on a broad base within the research community, permitting ecologists to incorporate new ideas and reevaluate research priorities.

ACTION ITEM #1: During the coming year, an organizing committee of the Ecological Society of America will plan workshops with the goal of coordinating the SBI with ongoing research efforts on global change and increasing research on the role of ecological complexity in global processes.

ACTION ITEM #2: During the coming year, an organizing committee of the Ecological Society of America will plan workshops with goal of developing an initiative on biological diversity that focuses on the ecological causes and consequences of patterns of biological diversity.

ACTION ITEM #3: During the coming year, an organizing committee of the Ecological So-

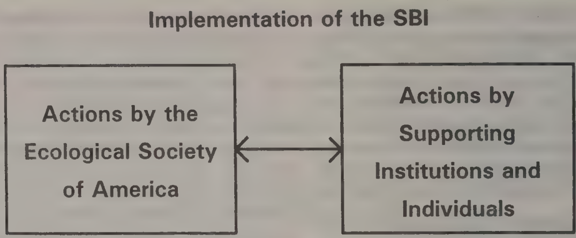

FIG. 6. Implementation of the Sustainable Biosphere Initiative will require a wide range of activities by many organizations (see text).

ciety of America will plan workshops with the goal of initiating a comprehensive program on sustainable ecological systems, emphasizing the underlying ecological processes that affect the sustainability of natural and managed systems.

These workshops will bring ecologists together with experts from related disciplines in the natural and social sciences and with resource-managers and environmental policy-makers to develop projects for immediate initiation.

EDUCATION

The environmental conditions that have mandated the Sustainable Biosphere Initiative also demonstrate the need for ecological education among citizens of today and tomorrow. Understanding and managing the biosphere requires ecological information. There are many strategies for addressing educational needs, such as working with the mass media to increase public awareness of ecological concepts and issues, making ecological literacy a goal of undergraduate curricula, and developing more interdisciplinary graduate degree programs that involve topics necessary for understanding the biosphere. Ecologically literate citizens should know not only the key concepts and principles of ecology, but also the basic processes by which ecological knowledge is acquired and the ways in which science and culture interact.

ACTION ITEM #4: During the coming year, the Research Agenda Committee of the Ecological Society of America will oversee the preparation and publication of a non-technical, public education document that articulates the importance of ecology and ecological research to society.

A diversity of strategies should be employed to address the educational needs of students, teachers, the general public, and decision-makers. These include: building ecology into pre-college curricula and teacher-training programs; making ecological literacy a goal of undergraduate education; and working with the mass media to increase public awareness of ecological concepts and issues. Educational efforts for fostering ecological understanding should build on and work with existing programs and initiatives in science and environmental education. Likewise, such efforts must be systematic and sustained.

ACTION ITEM #5: During the coming year, the Education Section of the Ecological Society of America will develop systematic, short- and long-term strategies for enhancing ecological knowledge among students and the public.

The Ecological Society of America should determine the human resources needed to conduct the ecological research proposed by the SBI and should examine specific vehicles to address the identified needs, including training grants and career development awards. Innovative professional education programs will be needed at the undergraduate, graduate, and post-doctoral levels to break down social and intellectual boundaries to interdisciplinary research, facilitate students' exposure to diverse biotic and professional environments, introduce students to conceptual advances in subdisciplines of ecology, and promote the incorporation of new technologies in students' emerging research programs. Finally, more opportunities are needed for established ecologists and other scientists to pursue interdisciplinary interactions, learn new techniques, and synthesize ecological knowledge.

Environmental Decision-making

Thousands of ecologically based decisions are made annually by policy-makers and regulatory agencies, land- and water-use planners, resource-managers, business and industry, consulting firms, and conservation groups. To be useful to decision-makers, ecological information must be both accessible and relevant to their mandates and responsibilities. The research component of the SBI is directed toward acquiring the ecological information (i.e., conceptual approaches, methods and tools, and data) needed to assess the status of ecological systems; to anticipate the impacts of management decisions and development options; and to conserve, restore, or manage ecological systems.

The application component of the SBI calls for the development of new institutional structures that will make ecological information more accessible to decision-makers. For example, collaborative programs between management agencies and academic ecologists offer benefits beyond the solution of important applied questions. Agencies benefit from the enthusiasm and innovative ideas of students and postdoctoral fellows; academics are challenged by urgent, complex problems. Training of students in both basic and applied realms will have long-term benefits for the development of ecology. Even the largest scale management projects involve mechanistic experiments and modeling studies that yield the short-term publications needed for career advancement within academia. Thus, the alliance of basic and applied ecology can invigorate academic ecology and strengthen the scientific basis of environmental assessment, rehabilitation, conservation, and management.

Application of ecological knowledge will require better communication between ecologists and decision-makers in all sectors of society. Knowledge transfer must be expedited and interdisciplinary barriers overcome. The experience of management-oriented professional societies in setting environmental priorities will be essential to open new avenues of communication.

ACTION ITEM #6: During the coming year, an organizing committee of the Ecological Society of America will begin to explore ways in which ecologists can become more responsive and bring their expertise more fully to bear on critical environmental problems. This committee will work closely with management-oriented professional societies, resource-managers, and other environmental decision-makers.

International Dimensions of the Sustainable Biosphere Initiative

The framework for this Initiative was developed in North America, but the research priorities and the environmental problems related to them are important world-wide. What is needed now is an extension of this initiative into an operational program of global scope.

ACTION ITEM #7: During the coming year, the Ecological Society of America will organize a meeting of leading ecologists from many nations of the world to evaluate the SBI and to begin construction of an operational framework for international cooperation.

At the same time there will be efforts to interact with governmental (e.g., UNESCO) and non-governmental (e.g., the International Council of Scientific Unions) international bodies that have programs closely related to the research agenda of the SBI.

Funding the Sustainable Biosphere Initiative

Meeting the financial needs of the SBI will require significantly increased funding from both public and private sources. Although there is a wide array of important and rewarding research questions, the SBI has identified those that are of the highest priority for the development of required knowledge and its application to conserving and wisely managing the Earth's resources.

Because of the broad importance of this Initiative, creative approaches to funding research will be required. Typically, public agencies such as the National Science Foundation fund basic research, mission agencies fund research that applies to problems of specific interest to the agency, businesses fund research to answer pressing industry questions, and foundations fund topics or themes of particular interest. The Sustainable Biosphere Initiative encompasses all of these missions, and as a result, must be planned and funded by a range of agencies and organizations.

Current administrative structures are insufficient to coordinate and fund the range of activities envisioned by the SBI. Consequently, it will be necessary to develop a new administrative structure that allows many agencies to support the integrated research program. To accomplish the needed coordination and funding, a variety of vehicles should be considered, including a new or existing interagency committee, a new national institute, or other administrative arrangements. This new organization would further develop research priorities within the SBI, coordinate funding strategies, and establish and implement procedures for evaluating the research progress of the Initiative.

ACTION ITEM #8: During the coming year, the Ecological Society of America will initiate discussions to develop an innovative framework to coordinate and fund the SBI. Emphasis will be placed on enhancing opportunities for investigator-initiated, peer-reviewed research in the context of coordinated programs that would fund both individual investigators and multidisciplinary research items.

Concluding Remarks

The ecological research agenda proposed in this document begins with the assumption that advances in understanding basic ecological principles are required to resolve many urgent environmental problems, continues with the identification of three priority areas for intense research efforts, and concludes with actions to be initiated by the Ecological Society of America to strengthen and expand research efforts in these key areas. The success of the Sustainable Biosphere Initiative will depend upon (1) the willingness of individual ecologists to participate in the proposed activities, to disseminate the vision of the SBI, and to plan and execute subsequent phases, and upon (2) the vision and abilities of policy-makers, funding agency administrators, government officials, business and industry leaders, and individual citizens to support, amplify and extend the actions we have initiated. At present, neither the funding nor the infrastructure in this country is sufficient to address the research needs described in this document. Moreover, achievement of a Sustainable Biosphere will require not only the acquisition of ecological knowledge via research, but also the communication of that information and understanding to all citizens and the incorporation of that knowledge into environmental, economic, and political decisions. Although there are formidable barriers to accomplishing these tasks, achieving a Sustainable Biosphere is one of the most important challenges facing humankind today. Time is of the essence. New technologies, widespread appreciation for the magnitude of environmental problems, and an increasing appreciation for the relevance of basic ecological research combine to provide an unprecedented opportunity to make significant progress in achieving a sustainable biosphere.

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IX. APPENDICES

Appendix A. Ecological Problems at Different Levels of Organization

Although many ecological problems cut across levels of biological organization, it is convenient to organize important ecological questions according to the appropriate level of organization. In listing the following questions, we have drawn extensively on the report of the Committee on the Application of Ecological Theory to Environmental Problems (National Research Council 1986).

Level of organization Ecological topic	Questions
Individuals	
Functional Morphology	What explains morphological variation within and among species? How does function follow form?
Physiological Ecology	How do physiological constraints limit the responses of organisms to their biotic and abiotic environments? What determines the physiological limits of an organism's response to stress?
Behavioral Ecology	How do individuals respond to information on the physical environment, resources, competitors, predators, or mates?
Ontogenetic Factors	What determines variation in the response of organisms at different stages of their life histories?
Individual Variability	How does the genotype of an individual affect its ecological interactions? What is the relative importance of genotypic and plastic variation in the response of individuals to environmental variation?
Populations	
Population Regulation	What processes have the most effect on population growth rate? Which of these are density dependent? How do density-dependent processes interact with other important processes?
Population Stability	What is the pattern of temporal variation in population size? Does the population density tend to return to some equilibrial level when displaced? Are there multiple stable points? Is there a minimal population size necessary to avoid extinction?
Dispersal and Migration	What regulates population dispersion and migratory behavior? How do populations respond to the frequency, scale, intensity, type, and duration of disturbance?
Population Structure	How do elements of population structure (i.e., genetic and age structure; patterns of variation in life history traits, physiology, and phenotypic plasticity) affect the ecological responses and interactions of a population? How does exploitation affect population structure?
Among Populations	
Predation, Parasitism, and Disease	To what extent do consumers or pathogens control a population? What is the relative importance of consumers or pathogens and extrinsic factors (e.g., stress, disturbance)? What is the role of fixed or inducible natural defenses?
Competition	What is the role of competition in the evolution and ecology of populations?
Mutualism	How do mutualistic interactions affect the response of a population to perturbations?
Indirect Effects	What are the potential indirect interactions in a food web? What is the relative strength of direct and indirect effects? How do higher order interactions and non-linearities in interaction equations affect the predictability of population responses to perturbation?

Level of organization Ecological topic	Questions
Communities	
Community Structure	How does community structure affect individual species embedded within the community? To what degree are some species interchangeable without affecting community processes? What do the collective properties of communities, including various community indices, tell us about their functioning? How is community structure affected by population dynamics of component species?
Biotic Diversity	What are the patterns, causes, and consequences of spatial and temporal variation in species diversity? What is the role of diversity of genetic composition, phenotypes, functional groups, demic structure, habitats, landscapes, and biogeochemical processes in ecological communities?
Succession	How do population interactions and other processes at the level of the individual organism combine to produce the relatively predictable sequences in community composition during colonization or re-colonization of an open habitat? What processes retard or accelerate the rate of succession in ecological communities?
Community Stability	How well do communities resist environmental forces that may perturb them? What properties of communities lead to resilience in the face of environmental change? How rapidly do communities return to their initial state, and what factors determine the rate of recovery? To what degree are communities resistant to invasion by alien species? How might we predict the ability of a new species to become established in a given community?
Ecosystems	
Flux of Energy and Matter	How does variation in energy and material fluxes affect ecosystem structure? What mechanisms account for the flux of energy and matter within an ecosystem? How does resource availability interact with other limiting biotic and abiotic factors to influence biogeochemical cycling?
Diagnostic Indices of Function	What ecosystem features serve as indices of ecosystem stress or "health?"
Cross-system Comparisons	How do ecosystems differ in structure, function, or response to perturbation or management? How does climate mediate ecosystem structure and function?
Ecosystem Mediation of Climate, Wastes	How do ecosystems mediate climate? What is the role of a given ecosystem in processing or sequestering anthropogenic wastes?
Among Ecosystems	
Landscape Ecology	How do land-use patterns influence the ecology of component systems, including all levels of ecological organization up to the scale of the landscape itself?
Responses to Environmental Change	What are the feedbacks between ecosystem and atmospheric processes, both within and among separate ecosystems, extending to a global scale? How does vegetation affect climate? What is the response of terrestrial, aquatic, and marine ecosystems to variation in CO_2? What are the effects of changing climate, atmospheric composition, sea level, ocean circulation, and ultraviolet insolation on ecosystem processes, including biogeochemical cycling?

Appendix B. Cross-cutting Issues in Ecology

A few general ecological issues are common to many specific ecological questions. This list identifies issues critical to elucidating ecological processes and enhancing the usefulness of ecology for solving practical environmental problems.

Interactions among levels of ecological organization. Virtually all questions in ecology explore how phenomena at one level are related to processes operating at other levels. Even if not explicitly identified, this issue must be considered in most ecological investigations. For example, responses at the level of the population, community, and ecosystem must be related to processes at the level of the individual organism, the level at which natural selection acts.

The effects of spatial and temporal scales. Processes and events at one scale in space and time have serious implications for, and may even control, processes and patterns at other scales. For example, intense competition for a limited resource could occur very rarely, yet dictate many characteristics of the competing species over long intervals of time.

The importance of heterogeneity or diversity at all levels of biological organization. Here we include questions concerning the role of genetic diversity, species diversity, and habitat heterogeneity at several nested scales; landscape-scale complexity; and many other aspects of ecological systems. For example, patchiness and heterogeneity of the environment may affect life history evolution, the coexistence of species, and the maintenance of ecosystem processes.

How multiple factors combine to affect ecological systems. It is critical to assess the cumulative impact of numerous factors at all levels of ecological organization. Physical and biological factors interact to influence ecological processes; a better understanding of this interaction would help to address larger problems. For example, organisms already stressed by crowding and the consequent intense competition for resources may often be more susceptible to mortality when subjected to additional stress. Multiple disciplines must be incorporated into ecological research as a means to understanding how multiple factors combine. Understanding the role of atmospheric processes, geochemistry in the soils, and the physics of the transfer of matter, heat, and momentum in water are all critical to developing the science of ecology.

The role of environmental variability. Ecological theory and empirical study alike have demonstrated the vast differences between systems at equilibrium and nonequilibrial systems. Consequently, priorities in ecological research include the magnitude and specific action of natural and authropogenic disturbance and the interaction of disturbance with other biotic and abiotic factors. Such research includes the issue of the ecological responses to environmental stress and the question of how structure and function of ecological systems at all levels reflect stress. Implicit in this general problem is also the question of how to detect change in ecological systems against a background of substantial variability.

Appendix C. List of Boxes

RISSER TESTIMONY

Attachment B

Roundtable

Biological research priorities—a sustainable biosphere

Disturbing examples of environmental problems around the world lead to the inescapable conclusion that human activities have begun to threaten the ability of Earth to support even current human life-styles. And for much of the world, current life-styles are far below adequate. A few years ago, statements that Earth's ability to sustain human populations is threatened might have been dismissed as unsubstantiated assertions from pessimistic, emotion-driven environmentalists. Now, however, that conclusion comes from the broader scientific community.

If Earth's ability to support both humans and natural functions of the biosphere is in jeopardy, then there is no higher priority for the attention of society. Moreover, because ensuring the continuation of a supportive biosphere requires research as a basis for making the most prudent decisions, there is no higher priority for research. As the Commission on Life Sciences of the National Academy of Science recently stated in a position statement to NAS president Frank Press, "We must simultaneously confront the financial consequences of substantial budget deficits and begin to restructure our scientific objectives toward the goal of assisting human societies to preserve their global biogeological life support systems."

The role of ecology

On simple reflection, it becomes clear that many of the environmental problems that challenge human society are fundamentally ecological in nature. The human population now numbers 5.2 billion, and it is increasing at the rate of approximately 1.8% each year. This population growth and the accompanying increasing use of resources are exerting tremendous pressure on Earth's ecosystems.

by Paul G. Risser,
Jane Lubchenco,
and Simon A. Levin

Ecological systems now support this human growth, but their ability to sustain human society is being degraded rapidly. Degradation of ecological systems is evidenced by increasing problems with the disposal of solid and toxic wastes, rapid rates of deforestation and watershed destruction, high rates of species extinction caused by human activities, and changes in tropospheric trace gases and in weather patterns. All of these environmental problems affect the United States. But as the world's population continues to expand with increasing demands for ever more scarce resources, and as standards of living decrease in developing countries, the deleterious effects of human activities on Earth's resources will grow at even faster rates both in this country and worldwide.

Because of the recognition that many of the world's most fundamental problems are ecological problems, the Ecological Society of America (ESA) concluded in 1988 that priorities for ecological research must be set for the closing decade of the twentieth century. The mere process of deciding to set research priorities at all is an unusual step for most scientific communities. That is, much of the United States's highly successful research effort is attributable to the intellectual curiosity of individual scientists, pursuing stimulating questions without having specific applications in mind. Much of what we know about the behavior of nature has been discovered by just such intellectual pursuits. Indeed, such research endeavors, driven by intellectual curiosity, must continue and be enhanced. At the same time, many of these basic research efforts must attain focus through their potential to solve the critical problems facing humanity. These interrelationships of basic and applied research, with their attendant synergisms, have long been a feature of the growth of science.

A sober analysis of US research efforts leads to several conclusions. First, there are finite amounts of human and financial resources that can be devoted to supporting research and therefore priorities must be set. Second, a variety of different approaches to resolving research questions has been successful. This diversity, involving the spectrum from single investigators to large integrated projects, must be maintained. Finally, many otherwise high-priority research projects will become meaningless if the biosphere is not sustainable. ESA accepted the challenge to set research priorities within the field and to contribute to discussions of priorities among scientific disciplines. The resulting research agenda described in this Roundtable was published as "The Sustainable Biosphere Initiative: an ecological research agenda" (Lubchenco et al. 1991).

A framework for ecological knowledge

The Sustainable Biosphere Initiative recognizes that sustaining Earth's ecological systems requires an understanding of those systems and calls for basic research to acquire this necessary ecological knowledge. Managing the biosphere also requires an improved and broad understanding of the functions of ecological systems, and hence the initiative emphasizes the importance of transferring ecological information to the public. Most important, sound decisions about environmental resources must be based on a solid research base; thus the initiative calls for the incorporation of ecological knowledge into policy and management decisions. The Sustainable Biosphere Initiative is a framework for the acquisition, dissemination, and utilization of the ecological knowledge that is required to ensure the sustainability of the biosphere.

Within the initiative's framework there are three research priorities:

- Global change: understanding how ecological processes affect local, regional, and global climate conditions, and how changes in these patterns of climate affect ecological processes. These many interactions involve the atmosphere, soil, and water and may be driven by, for example, changes in climate and in land use.
- Biological diversity: understanding how the patterns of genetic, species, and habitat diversity are affected by human activities and natural phenomena. In particular, biologists need to understand how diversity affects the behavior of ecological systems and how ecological processes control biological diversity.
- Sustainable ecological systems: understanding when natural and managed ecological systems are stressed to the point that they are no longer capable of being sustained, how to restore damaged systems, and how to manage ecological systems so that they can remain productive to support natural process and the human population.

Although there is an ongoing, large program on global change (US Committee on Global Change 1990), insufficient attention has been paid to the ways in which ecological conditions and processes control global processes. Species and habitat diversity, patterns of distribution of ecological assemblages of organisms, and differences in the productivity and chemical storage capabilities of different types of ecosystems all influence global processes, such as fluxes of trace gases between Earth's surface and the atmosphere, transfers of materials from terrestrial to aquatic systems, and many other biospheric functions in Earth's system. For example, as global concentrations of atmospheric CO_2 increase, certain groups of plants become much stronger competitors and would be expected to become far more common. These new combinations of species may affect how nutrients are stored in the biosphere or how trace gases are released to the atmosphere.

Within the topic of biological diversity, much of the current effort is rightly devoted to cataloging the species in various habitats and to protecting pieces of landscape that contain high levels of biological diversity. These important efforts must be continued, and they lay a part of the groundwork for the research proposed in the ESA initiative (Committee on International Science's Task Force on Global Biodiversity 1989). However, ultimately, two vitally important research topics must also be addressed.

First, to understand and make justifiable decisions about preserving biodiversity, it will be necessary to discover the extent to which patterns of biological diversity determine the behavior of ecological systems (e.g., the ways in which different degrees of species diversity control how the biosphere responds to climate change, how this diversity relates to the rates at which nutrients flow across the landscape into rivers and oceans, or how various ecosystems respond to pollutants). Second, it will be necessary to understand how ecological processes (e.g., seasonal dynamics of soil, water, and available nitrogen) influence biodiversity. Only when these relationships are known will it be possible to develop management strategies for maintaining natural and managed ecological systems that support the biosphere.

Unlike the first two research priorities, in which there are currently some focused activities, the third priority—evaluating and managing sustainable ecological systems—represents a major new integrated program. Here the focus is on understanding the underlying ecological processes in natural and managed ecosystems for the specific purpose of prescribing the most effective restoration and management strategies to ensure the continuation of Earth's ecological systems.

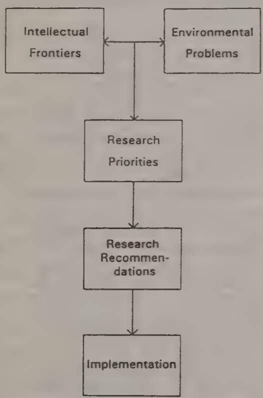

Figure 1. Intellectual frontiers and environmental problems were the dual criteria used to establish research priorities. Essential components of research in the priority areas have received insufficient attention. These key components form the basis of the research recommendations. Implementation of the recommendations requires specific actions by the Ecological Society of America and by other supporting institutions.

New needs for interdisciplinary efforts

Certainly there are significant efforts today designed to develop individual sustainable natural resources (e.g., sustainable forestry or sustainable agriculture). However, these isolated and fragmented research efforts are simply inadequate for dealing with the complexity of all the needed sustainable systems, which may involve multiple resources and several ecosystems, and for considering ecosystems that occur over large spatial scales. Moreover, much of the current research focuses on managing commodity-based systems. There is virtually no attention being paid to the sustainability of natural ecosystems that are characterized by goods and services without current market value.

Addressing the topic of sustainable ecological systems will require entirely new ways of organizing research projects, because the topic crosses the responsibilities of many management agencies and involves the integration of social, physical, and biological science. The Sustainable Biosphere Initiative (Lubchenco et al. 1991) does not simply assert these research priorities. Rather, these priorities follow logically from a clear description of the intellectual frontiers in ecological science, which are then matched with the most critical environmental problems (Figure 1).

The same environmental conditions that have mandated the research priorities of the initiative also lead to a need for education to ensure that there is sufficient ecological understanding among citizens of the United States and the world. In addition, there is an imperative to make ecological information conveniently accessible, practical, and relevant to the needs and specific mandates of decision makers. Although the Sustainable Biosphere Initiative was initially developed in North America, the environmental problems, and thus the associated required research, education, and decision-making priorities, are worldwide in importance.

The environmental conditions and the needed research agenda are not the exclusive domain of ecologists. Indeed, the Sustainable Biosphere Initiative could have been put forth by any number of scientific disciplines. Ecologists have unique knowledge and skills that allow them to conduct research on these topics, but interactions with other disciplines will be necessary for the required comprehensive approach to these urgent environmental problems.

Studies of global change, for example, cut across many fields, including ecology, atmospheric chemistry and physics, oceanography, hydrology and geology, human demography, and economics. Likewise, addressing biological diversity will require collaboration among ecologists, taxonomists, conservation biologists, policy makers, planners, political scientists, and economists.

Ecology, in many ways an interdisciplinary science itself, will play a critical role in accelerating the development of new interdisciplinary approaches to the study of these environmental problems. Developing prescriptions for the sustainable human use of Earth's resources will require new and more intensive alliances among ecology and other disciplines, including resource management; agronomy, forestry, soil science, and other environmental sciences; epidemiology and demography; and economics and planning.

Implementing the initiative

The magnitude of the Sustainable Biosphere Initiative will be even greater than the Human Genome Project or the Earth Observing System. The Sustainable Biosphere Initiative crosses more disciplines and involves the mandated responsibilities of more federal and local agencies; its success will require entirely new ways of conducting scientific research. At present, neither the funding nor the infrastructure in the United States is sufficient to address the research needs. This initiative, of necessity, will transcend both public and private institutional boundaries and will involve extensive collaborative programs.

Implementation of the Sustainable Biosphere Initiative requires the following steps:

- Systematic analysis of all the pertinent federal agencies to determine which parts of the initiative are now funded but require additional funding and which parts of the initiative are not currently included in existing research programs.
- Estimation of the cost of implementing the initiative as it develops into an operational program during the next five years.
- Development of an interagency process (which will recognize the involvement of several federal agencies and many scientific disciplines) for organizing the necessary funding.
- Establishment of a combined governmental and nongovernmental organizational structure to manage the coordination of the initiative and to assess its progress.

The Sustainable Biosphere Initiative has been formally discussed with all the relevant US federal agencies, many professional societies of related disciplines, and with several countries throughout the world. Initial discussions with US agencies have begun to describe the required expansion of existing research programs and the development of new programs. Preliminary estimates are that an additional $500 million annual budget will be required for US research by the fifth year of the developing Sustainable Biosphere Initiative.

Conclusions

The success of the Sustainable Biosphere Initiative requires a major effort by the scientific community and broad support among those organizations that support research and use the results of research in managing the biosphere. Although there are formidable barriers to accomplishing the initiative, achieving a sustainable biosphere is the single most important challenge facing humankind today.

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Mr. BOUCHER. Well, thank you very much, Dr. Risser, and the subcommittee expresses thanks to both witnesses today for their very helpful testimony.

You represent two disciplines in which priority setting internally has been successful. I'm not aware of successful efforts to the extent of your disciplines and other disciplines at the present time. I wonder if there's something inherent in the nature of astronomy on the one hand, and ecology on the other, that makes it somewhat easier for you to set those priorities internally that perhaps would not apply in the case of chemistry or physics or biology or some other discipline.

Dr. Bahcall, do you sense that there's something inherent in the nature of astronomy that makes it easier to set priorities within your discipline?

Dr. BAHCALL. Well, if I may be allowed, it's more interesting, our subject—

[Laughter.]

But on a serious vein, some of my closest friends are physicists. I'm married to a physicist, I was trained as a physicist, I was a professor of physics until I moved to Princeton, and yet my friends in physics say we could never do that, and friends in chemistry have said the same thing.

I don't think that's correct. There were a significant fraction of our committee—I would say more than a third—were people who were physicists by training, such as myself. There were two Nobel prize winners in physics who do physics now who were members of our astronomy committee. But as you know, the SSC and the other major physics projects in that arena do not receive the same numerical prioritization that things do in astronomy, and I think they're are just two reasons. One is there's no incentive for them to do so, and second, they never had that experience before.

For astronomy, we've had the experience of doing it before, and it worked out very well for us. We found that it helped us get a larger fraction of our projects funded. We have that as our incentive now, and it works for us. I think if there was a similar incentive for other fields, they would do it, too.

Mr. BOUCHER. Let me get a sense of what it is that you set priorities for. Within the field of astronomy, as I understand it, you have essentially ranked the capital projects that needed Government funding for the coming decade.

Dr. BAHCALL. That's correct.

Mr. BOUCHER. And so you're dealing with a finite set of physical facilities. I would suggest to you that it might be somewhat easier to rank those physical facilities than it is in in the field of chemistry, for example, to rank literally thousands of proposals for research, and that for that reason maybe it's inherently somewhat easier for astronomers to do this than it is for scientists in other disciplines.

Is there anything to that train of thought?

Dr. BAHCALL. Well, I'm sure there is, and I'm sure chemists would like you to continue to think that way. But I don't accept it. I don't think that—

Mr. BOUCHER. Tell us why that's wrong.

Dr. BAHCALL. It's not that it's wrong. I have a different view.

In astronomy, we have to rank the use of telescopes or facilities that have to do with exploring planets nearby, or exploring life in our own solar system, out to the very beginnings of the universe, at the very reaches which man can even envision. We have to rank projects which are a billion dollars in scale or thousands of dollars in scale. We have telescopes that are in space; we have telescopes that are spread internationally; we have telescopes underground, believe it or not. We have the greatest, I think, diversity of interests in our discipline, and the greatest scope for subjects. There's nothing bigger than the universe for the scope of the interests that we have. And yet we set numerical priorities not just among capital projects, Chairman Boucher, but also we place in the same arena the development of human resources, the infrastructure, in the same sense that we judge new capital investments.

And, in fact, to the surprise I think of many people in Washington, the projects—The highest priority from ground-based research was not any new facility; it was the strengthening of the support for the facilities that we already had. And our committee said very clearly, if we don't get any of the marvelous new toys that we're asking you for, please at least give us the ability to use the ones that we have now in an intelligent and modern way. So we took hard choices among things which were not strictly tangible. We put a very high emphasis on the development of the programs to support individual investigators, again above what we supported for major facilities.

Mr. Boucher. I think your success is very commendable and, in fact, it is your success that leads us to inquire as to the appropriateness of extending this kind of priority setting into other kinds of disciplines.

Dr. Risser, would you care to comment?

Dr. Riser. Yes. Thank you, Mr. Chairman. Astronomy, clearly, is interesting; ecology is more important.

[Laughter.]

I don't really mean to be jestful about that. I think that what made the Ecological Society come together was just that point, the recognition that, in some ways, if we don't solve the environmental issues, these other issues become really of not so much value. So there was a recognition that so many of the environmental issues today are founded in ecological sciences, and unless the ecological community comes forth and says what's the most important priority, then there's not much hope for lots of us. And I'm not by nature an alarmist. It simply is a responsibility that that science has to essentially this country and to the world. So I think it was that imperative that really made this community come together and reach these priorities.

I think, as John has said, these science disciplines are very, very broad; that is, the ecologists worry about all the way from using remote sensing to measuring trace gas emissions from the whole globe, to the genetics, for example, of individual species. So it's a very broad process. In the setting of priorities, there were lots of different ideas which were discarded, but in the end, the recognition that these three priorities were, by far, the most important, let this community come together as it never has before.

Mr. BOUCHER. Do you acknowledge that it might be somewhat more difficult to do this in the other disciplines, in chemistry, physics or the like, or do you share Dr. Bahcall's idea that, if proper incentives are provided, that it can happen there as well?

Dr. RISER. I share the same impression. That is, I don't believe that other disciplines are—

Mr. BOUCHER. My impression or his impression?

Dr. RISER. I'm sure Dr. Bahcall's, that other scientific disciplines are not really, inherently, any more wider, for example, than our two disciplines. But I think it is the incentive—He talked about one kind of incentive, which was essentially a practical incentive, in terms of making things come together, and I talked about essentially a topical incentive. I think it takes an incentive—you've heard about two different kinds of incentives, but it takes incentives, but there is no inherent reason why those disciplines can't come to the same kinds of conclusions.

Mr. BOUCHER. Let me see if I understand Dr. Bahcall's recommendation for incentives. As I understand your statement, you would have a certain percentage of the funds that are allocated by the Federal Government for research in given disciplines set aside for the priorities that are set within those disciplines.

Do you intend for that to have any kind of cross-disciplinary effect, or is it only within the discipline itself that that set aside would apply?

Dr. BAHCALL. I think it could work within disciplines, which it does already in astronomy. I think it could work equally well across disciplines. I think if you gave Dr. Risser and myself the responsibility for a fixed sum of money which had to be allocated between astronomy and ecology, we could, as citizens and as scientists, and with the appropriate advice of our colleagues, agree on a consensus recommendation for that and it wouldn't all be astronomy and it wouldn't all be ecology, but it would be an intelligent allocation of limited resources. If we had that incentive, I think we could achieve a consensus agreement among scientists properly charged.

Mr. BOUCHER. What kind of structure would you put together to reach that kind of consensus? I could imagine just enormous practical problems. Once the priorities are ranking within a given discipline, if then there is a pool of money set aside that is designed for cross-disciplinary funding, how do you, as a practical matter, go about reaching those cross-disciplinary decisions? What kind of structure would you suggest to us by which that could occur?

Dr. BAHCALL. I was enormously struck this morning during Dr. Risser's testimony by the similarity between the activities, which were entirely independent, that he participated in and that we participated in. I think the first rule was a very common—was very important. Their activities and our activities were independent of the agencies, and I think it's very important that it be the people who have the ideas and the science who do it and not the people who are pushing individual agencies or programs within agencies.

But, other than that, I think you could take advantage of the national research, which is the National Research Council, and ask them to have a wide-ranging set of people who would be charged with providing a limited set of exemplary projects which were recommended by a cross-disciplinary group as the most important things to do in science, and that there be a limited amount of funding taking out of the other funding that would go to that type of activity. You would see superlative proposals then because there would be an incentive for it.

Mr. BOUCHER. So you would place some decision-making function then with the National Research Council, with respect to the cross-disciplinary aspects of priority setting?

Dr. BAHCALL. Well, I wouldn't say decision-making. I would say advisory to the Congress or to the executive branch. But they would be charged with—this group would, in my view—be charged with the responsibility of recommending a small number of prioritized projects and recommending that for the consideration of the Congress and executive branch.

Mr. BOUCHER. Dr. Risser, let me get you to comment on the general subject of incentives, and if you want to address the question of structure and how those incentives might be awarded, that would be helpful, too.

Dr. RISER. Thank you, Mr. Chairman.

I think that incentives are, in fact, a reality of this process, and so that they have to be managed properly. Clearly, incentives can help these interdisciplinary and multidisciplinary kind of projects. So I agree that having a structure in place is important.

There are, as you know, in some Federal agencies, attempts to do that at the present time. They work to different levels of efficacy, but nevertheless there are attempts, for example, in the National Science Foundation to try and bring those about.

Turning to the third priority, the one of sustainable systems, will require that kind of effort if we're ever to be successful. I agree that the National Research Council might be helpful, particularly in describing the process for bringing together these ideas, although I agree that I don't think the NRC should be the recommending body in terms of national research priorities. In fact, that ought to be done within a consultive activity between the scientific communities and the Federal Government agencies.

I guess I would add one caution, and that is, as a scientist, we always hesitate if we use a process which truncates too severely the various options that we address, so that I think certain amounts of funds ought to be used as incentives for these interdisciplinary kinds of projects. But we should never have such a restrictive recipe for funding science that we don't allow the scientific process to be curious and to explore various options.

Mr. BOUCHER. Let me ask a somewhat broader question. Suppose that the National Science Foundation just decided that its funding in the field of chemistry, for example, would be allocated virtually entirely in accordance with the priorities that are set by those who are practicing within that discipline. What would be the practical effect of that?

This goes well beyond your suggestion, Dr. Bahcall, that only a certain percentage of a budget be set aside as incentives. Let's suppose that we really have confidence in this priority-setting mechanism. We like the idea of those within the discipline setting the priorities, and we're simply going to allocate all of our funding to the priorities that are so established. What are the pros and cons of taking that kind of approach?

Dr. BAHCALL. Well, sir, that's what happened in response to our recommendations to NASA. There was a dialogue between our committee and the Space Science section at NASA. They responded in detail to each of our recommendations and said how they would implement them. And they asked the National Research Council to have our committee review that set of recommendations whereby they would implement our priorities, and they asked us to review that to see if they had correctly interpreted our recommendations—and they had. In fact, they had been wiser than we had in understanding how to do some of the things.

So NASA did, in fact, accept the recommendations of the Astronomical and Astrophysics—

Mr. BOUCHER. So you would see no harm in that kind of approach?

Dr. BAHCALL. I think it was very healthy, and I think it's—

Mr. BOUCHER. Would it be equally healthy across other disciplines?

Dr. BAHCALL. I don't see any—I think the alternative is to have people less expert, less knowledgeable, and less concerned about the subject, setting the priorities.

Mr. BOUCHER. Let me be devil's advocate with you for a moment. Let's suppose that kind of structure were put in place, and essentially all of the funding for a given discipline was awarded in accordance with the priorities set internally within that discipline.

Does that not limit flexibility to respond to unintended or unexpected circumstances, to fund research that only becomes apparent after some of these initial decisions are made? The money's gone by that point. You have to wait another year under normal circumstances. Would that not be a problem that we should consider?

Dr. BAHCALL. Well, we worried about that, sir, when we made our recommendations, and we made recommendations only in the largest categories, in the categories where quick response was important and possible, and where agency peer review was appropriate. We have exemplary projects that we thought were important in the contemporary time frame, but we specifically said don't take these seriously after two or three years. Look for new projects.

So I think you can build into this kind of program the appropriate quick response to smaller projects which need to be done on a fast time scale. It's the things which are big projects which are the ones that I think most concern, where you're most concerned about, where we have to have a consensus about a smaller number of highest priority—

Mr. BOUCHER. Dr. Risser, let me ask you to comment.

Dr. RISER. Yes, thank you.

I think I make two points. First of all, the suggestion that the scientific community doesn't now have input to the Federal agencies is not true, that there is an elaborate structure for advice, and I think in many cases that structure works quite well.

Secondly, I think there is also the expectation sometimes that the scientific community might go one direction and the Federal enterprise might go a different. I think that's really not so likely, that at least in our experience, the kind of priorities that came from the scientific community are almost precisely the same sort of priorities which would have come from the Federal agencies. So I think there's a better communication at the scientist level than we might have expected, and so there's a greater convergence.

I would also reemphasize, however, my caution—which is the same as yours—and that is, that we not essentially have a regimented, pedigree here for how we spend all the money and that we have to retain enough for the kinds of scientific projects which don't fit in the current priorities but become important in the future.

Mr. BOUCHER. Thank you very much.

The gentleman from California.

Mr. PACKARD. Thank you, Mr. Chairman. I'll be brief. I took more time on the first panel and so I'll be brief on this panel. But one question to each of you, one to you, Dr. Risser.

Chemists and physicists and engineers, and those that are more in the physical sciences, and research projects that relate to those kind of sciences, may have a different prioritizing system or a different set of priorities in terms of what the goal and the objective of their projects would be, or their priorities would be. For example, they may be more oriented toward economic benefits. Whereas research in your areas of your interest would be more involved in assisting the establishing of ecological and environmental policy. Often one would tend to return to our society economic benefit, where the other may be construed to literally drain or take from our resources in order to preserve and protect something that's very important to our society.

Are there differences in the kind of prioritizing or the kind of criteria that would be used in setting those kinds of priorities, and are they competing one with another, in your judgment?

Dr. RISER. Mr. Chairman, Mr. Packard, I think that that's a very important point, and I don't think they're competing. Let me make again two comments.

First of all, that the third of the three priorities, which in some ways is the most important one—on sustainable systems—brings together not only the ecological values but also the economic values as well. So it's precisely the point that you're making which asks that, in fact, the scientific community move from a single issue of simply preservation, as you suggested, to one which manages the system in a sustainable kind of way, recognizing that there are many demands made on it—some of which are ecological demands, some of which are economic demands. So it's that combination which is so important in this whole initiative and which is embodied in that third initiative.

Secondly, I should point out that the science really of economics and ecology is becoming closer and closer together, in terms of how we go about setting these priorities, so I think the days in which we had a separate decision based on economics and a separate one based on ecology, those days are over, that, in fact, what's happening in the scientific community is a bringing together or both of those disciplines to try and address the kind of issues that you're raising.

Mr. PACKARD. And you feel that that transition has been particularly noticeable in the last decade or so?

Dr. RISER. Indeed, I do. That's correct.

Mr. PACKARD. Thank you very much.

Dr. Bahcall, you gave very short allusion to the question that I brought up on the previous panel, and that is, in your judgment, there are political and, as you call it, pork barrel projects that enter into the picture. What needs to be done or could be done that would better insulate the process, the prioritizing process, from the political emphasis, and in fact, has the political prioritizing been an impediment toward what the scientists feel and certainly what the academic community feels is a good prioritizing system? Has the political process been an impediment for that?

Dr. BAHCALL. Mr. Packard, I think there are examples where the political process has distorted the scientific priorities and that's had a major impact from time to time in the physical sciences. But I'm not sure that it's always entirely inappropriate. That is, the country and you have other criteria that you must consider in addition to scientific priorities. We're expert only in the scientific priorities and I think you, the Congress and executive branch, have to take a wider perspective at times and take other things into account.

But I would like, strictly as a scientist, to see some greater insulation. I think the idea of setting aside a certain fraction of the funding for only projects which received a discipline-wide or a cross-discipline recommendation by an appropriately constituted prioritizing body would help to insulate us from the spending which is influenced simply by political considerations.

Mr. PACKARD. I appreciate your comments on that. I think, in defense of our science prioritizing process, we do a better job than we do in many of the other areas of Government spending. I serve on Public Works and Transportation and, obviously, that is a very, very...

Dr. BAHCALL. I see you're searching for the appropriate phrase.

Mr. PACKARD. Right.

[Laughter.]

You're right. I don't want to offend my chairman over there. But certainly special projects and political decisions are made much more readily than they are in other areas, and that may suck up a lot of moneys that would be better prioritized. I don't think we have that—certainly not the level of problems in this, our science areas, because I think there is a desire on the part of most people to genuinely choose from a merit position what are your best scientific research projects, and then politics sometimes gets in the way of that. But it is not often the driver of all that we do like it is in some other areas. But I appreciate your comments and your testimony.

Thank you very much, Mr. Chairman.

Mr. BOUCHER. Thank you very much, Mr. Packard.

The subcommittee extends its thanks to both of these witnesses. You've been very helpful to us today. We appreciate your comments and your recommendations.

Before proceeding to Panel 3, the Chair is going to declare a five-minute recess of this subcommittee. We'll convene again at one o'clock.

[The subcommittee was in recess.]

Mr. BOUCHER. The subcommittee will come to order.

We will welcome now our third panel of witnesses for the day, consisting of Dr. Kumar Patel, the Executive Director for Materials Science and Engineering, of AT&T Bell Labs, and Dr. D. James Baker, President of the Joint Oceanographic Institutions from Washington, D.C. These witnesses this afternoon will discuss priority setting by the Federal Coordinating Council for Science, Engineering and Technology, otherwise known as FCCSET.

We welcome you. Without objection, your prepared statements will be made a part of the record, and we would ask that you keep your oral summaries to five minutes. Dr. Patel, we'll be pleased to begin with you.

STATEMENTS OF C. KUMAR N. PATEL, EXECUTIVE DIRECTOR, RESEARCH, MATERIALS SCIENCE, ENGINEERING, AND ACADEMIC AFFAIRS DIVISION, AT&T BELL LABORATORIES; AND D. JAMES BAKER, PRESIDENT, JOINT OCEANOGRAPHIC INSTITUTIONS INCORPORATED

Dr. PATEL. Mr. Chairman, Mr. Packard, I'm really pleased to be here this morning and share with you my views on the subject of priority setting in research, especially in materials and processing sciences. I'm going to focus my comments specifically in the area of these two disciplines as they apply to industry. I have four priority setting criteria that I would like to talk to you about. These have worked remarkably well in industry for many years.

These four criteria include conceptual versus maintenance research; second is knowledge generation versus value creation; third, customer identification and customer involvement; and finally, measures for evaluation of cost-effectiveness of research.

The first one, is the types of research. The first category of the type of research is what we call conceptual breakthroughs in phenomena and materials leading to new technology which displaces existing technology. Sometimes this is also called "killer" technology, and I'll come back to that in just a second. In any case, the new technology results in new products and services.

An example of a killer technology is the transistor, which completely displaced vacuum tube technology some 30 years ago; lasers, which help displace all the wire-based and microwave-based communications in the last ten years.

The new research is often likened to seeking the "silver bullet". Research on high temperature superconductors is a prime example of the first type of research.

The second type of research supports existing technologies. I call that "maintenance research", and it provides incremental, or five to ten percent, improvements per year in existing product quality. Many of our overseas competitors have become experts at this type of science. But I would like to point out that this research is not development, because there are many fundamental issues associated with new materials, with new processing, that deal with current technologies.

The second criterion for priority setting in science in industry is knowledge creation versus value creation. An industry needs more than just discoveries or inventions. It needs usable technology on which commercially definable markets can be built. The same can be said about national science policy, also. This part of technology creation is what I call value creation. Knowledge creation and value creation are the two principal yardsticks that we use in setting priorities in science.

The third criterion is identification and involvement of customers. For knowledge creation, the customer is the scientific enterprise and publication of results is an important vehicle for serving this customer. For value creation, customers include development engineers, manufacturing personnel, the ultimate user, or perhaps even the nation. For maximizing benefit to the society, we must alter the paradigm regarding who should be included in the priority setting process for science.

In addition to Congress, Executive Branch, Federal agencies, and academia, we need to include the industry, because it is that part of our society that takes science and converts into wealth.

Industry is a customer which derives value from national support of science, just as I mentioned. Here we consider both knowledge as well as value creation aspects. Examined in this light, many of the megaprojects neither meet the future knowledge needs of the industry, nor do the research results provide any identifiable direct value.

The final criterion is the output measure of science and judging its cost-effectiveness. Evaluation of research productivity requires a multiparameter scheme. Many of the parameters are not quantifiable and require an intimate knowledge of science. I, for one, do not consider papers published to be even a rough guide of productivity because the publication rate is very much subject dependent and often has turned out to be fashionable.

A second measure of federally funded science is the quality and cost-effectiveness of the production of future scientists and engineers. Close technical interactions between industry and academic institutions help rationalize what universities produce and what industries need. Such active participation, unfortunately, occurs only when there is a strong overlap of industrial science with academic science.

It is gratifying to note that materials synthesis and processing are beginning to be recognized as being important. Years of benign neglect and often active down-playing of the importance of new materials have taken its toll. New phenomena and new device concepts will require materials which do not exist. Without new materials, the capability of U.S. to capitalize on its scientific advances in new phenomenology will be seriously compromised.

Finally, I would like to comment on the perception of the difficulty faced by young investigators in academic science, something that we have touched upon earlier today. The situation of constrained funding of science would likely change as the national economy improves, but the diminished expectations of the current generation of young faculty members could be detrimental to the long-term health of science and the U.S. economy as a whole.

Let me conclude. Instead of the plethora of different and often inconsistent criteria for priority setting, I am proposing that we ask four simple questions: what is being done, what is the outcome, for whom it is being done, and how well it is being done.

Support of science, especially physical sciences, should be viewed in the context of satisfying the long-term technological needs of the Nation. If a project does not pass this filter, it should be given a lower ranking than one that does.

It is time to move carefully but rapidly and decisively with priority settings in science, especially amongst megaprojects in a cross-cutting manner, among small science projects within a given field, and especially between megaprojects and small science. In science, not everything that can be done needs to be done. Not everything that needs to be done deserves to be done now. Only by setting national priorities can we do the right thing at the right time.

Mr. Chairman, I want to thank you for your attention. I will be happy to answer the questions.

[The prepared statement of C. Kumar N. Patel follows:]

STATEMENT BY
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HOUSE SCIENCE SUBCOMMITTEE
APRIL 7, 1992

Mr. Chairman and members of the Science Subcommittee of the U.S. House of Representatives:

I am very pleased to be invited to discuss with you and share with you my views on the subject of priority setting in research and development activities in materials and processing sciences in an industrial environment. My discussion will be focused on four priority setting criteria that have worked remarkably well in the industry over many years. These include considerations of conceptual versus maintenance research, knowledge creation versus value creation aspect of scientific research, customer identification and customer involvement in setting research priorities, and cost-effectiveness of resources invested in support of research. I trust my remarks today will provide useful insights into the overall subject of priority setting in science.

Materials and processing sciences are two important core competencies forming the infrastructure of manufacturing and are intimately tied with the overall collection of manufacturing activities. Manufacturing is most efficiently carried out in industries. Further, materials and processing R&D must contribute to improvements in product quality and cost for it to have an important role in an industry. Before I go too far into the specific aspects of priority setting, it would be helpful for me to define the term "Research" as it applies to industrial science as a whole. The definition of the two types of research activities constitutes the first of the priority setting criteria. The conventional definition of research involves the search for conceptual breakthroughs in new phenomena and new materials which hopefully lead to new technology (sometimes also called "killer technology") which displaces existing technology. This new technology, then, is expected to result in new products and services that would open up new markets and lead to potential profits for the industry. An excellent example of such a revolutionary technology is the transistor whose invention some forty years ago led to a rapid demise of the vacuum tube technology. However, this is too narrow a definition of research, especially in an industrial environment. In industry, research also includes exploration of science in support of existing technologies. I would call this "maintenance research". It is expected to provide the incremental, 5 to 10 %, improvement per year in the existing product and service quality and a reduction in cost year after year. Materials and processing sciences are particularly good candidates for the second kind of research activities. It allows an industry to retain and perhaps expand its market share in a particular product/service segment, leading to continued improvement in the bottom line performance of the industry. It is this improvement which generates the needed profits for the industry to invest in the "conceptually new" research activities. Conceptually new research is often likened to seeking the "silver bullet". Many of our overseas competitors, on the other hand, have become experts at the scientific research which is designed to make the 5-10% yearly improvements in manufacturing processes, and have placed many of the U.S. industries a competitive disadvantage. Often, maintenance research is viewed as development. Nothing can be farther from reality. Improving processing science infrastructure of silicon integrated circuit manufacturing, for example, involves exciting materials challenges and has just as much intellectual content as research being carried out in an area where focus of the end utilization is less well defined.

It is also important to notice that neither the time frame nor basic versus applied fraction of science support determines the definition of conceptual and maintenance research. The conceptual research is driven, to a large extent, strictly by scientific and intellectual opportunities. Maintenance research, on the other hand, is driven by the long term technology focus of the corporation. The scientific opportunity driven research will lead to technological opportunities not yet recognized. Focus driven science, on the other hand, allows a corporation to more fully explore the long term viability and limits of the current technologies from a very fundamental viewpoint.

Both the conceptually new and maintenance research activities have the requirement of new knowledge creation. However, an industrial corporation (as well as a nation) needs more than just inventions and discoveries. It needs usable technology on which commercially definable market position can be built. In other words, value creation is an equally important function of scientific research in an industry. Thus we have the second of the criteria for cross disciplinary evaluation of various potential opportunities for investment of R&D resources. Knowledge creation and value creation are the two principal yardsticks that an industry uses in setting priorities in science. These twin criteria should also be useful when setting priorities in science at the the national level.

It is appropriate to ask about the relative importance of the knowledge creation in comparison to the value creation yardsticks. In industrial research priority setting, we have recognized that knowledge creation without any attention to value creation is likely to lead to a divergence in the long term technology needs of the corporation and the direction of scientific research. Such divergence, then, results in a reluctance on the part of the corporation to adequately fund the scientific research. On the other hand, value creation without adequate attention to knowledge creation robs the scientific enterprise of the intellectual excitement that has traditionally drawn the best of our young people into scientific research establishment. The OTA report correctly identifies national objectives, research goals, and agency missions for evaluating the total

Federal research portfolio. Put into my terminology, research goals to a large extent are subsumed in the knowledge creation part, while the agency missions help define the value creation part. National objectives which may include discoveries, inventions, education and training of the next generation of scientists, and providing technology directions for improving international competitiveness cover both the knowledge and value creation requirements. It is interesting to note that the national objectives which can only be enunciated by either the congress or the executive branch (or both) are the least discussed and the least understood. It is my opinion that many in the executive branch view setting well-defined and well-detailed national objectives to be synonymous with setting a technology policy for the nation. A carefully and clearly spelled out national objectives should be the starting point for any priority setting process. Without such an overarching vision, details of the priority setting process just will not achieve the coherence necessary for meaningful long term impact.

Every human activity must have a customer. Often the customer can be the individual herself or himself. But more often than not there is also an external customer. Scientific research, too, must have customers. Knowledge creation part of the scientific activities has among its customer set the scientific enterprise as a whole and publication of results is an important vehicle through which this customer set is served. In the second part of the desired state of science, the value creation part, customer set includes the development engineers, manufacturing personnel, and the ultimate user (consumer) community. In an industrial environment, support of research often is not derived directly from the customers of research. Nonetheless, we have found that close interactions with the customers are very important in early identification of limitations of current technology and the underlying science base. These interactions shape the third criterion utilized for evaluating different possible research directions for major investments.

Hitherto, the national priority setting in science has been driven to a large extent by only the first of the customer set since the notion of the importance of value creation through science is not well recognized and certainly not well articulated. And therefore the priority setting activities have been driven by the single set of customers. For delivering the maximum return to the society, we must alter the paradigm regarding who should be included in the priority setting process. The end customers, including industrial product and service development scientists, manufacturing engineers, and the consumers, should have an input in the priority setting process in science.

When an industry looks at the national research scene, it views itself as a potential customer who could derive value from the investment of national resources which are allocated to various scientific activities. Both the knowledge and value generation aspects are simultaneously considered. Examined in this light, many of the megaprojects neither meet the future knowledge needs of the industry, nor do the research results provide any identifiable direct value. It is not my intention to discuss any specific megaproject. It suffices to state, however, that an explicit project by project and industry by industry correlation can be made leading to an evaluation of the overall importance of the project to the industry using these clearly explainable and generally accepted criteria.

Much has been said about the cost of doing research and how one quantifies productivity of science. Industry rarely uses the number of published papers as a criterion for determining productivity of a given line of enquiry. Evaluation of research productivity requires a multiparameter scheme. Many of the parameters are not quantifiable and require an intimate scientific knowledge of the field. It also requires an ongoing understanding of trends and accepted limits of the field. How the research result affects the previously accepted limits and how it pushes back the boundaries of knowledge is more important than sheer number of papers. I do not consider number of papers or number of Nobel Prizes to be even a rough guide to productivity because publication rate is subject dependent and is a social phenomenon which has seen a "grade inflation" by it having become acceptable to publish results of a study in small incremental bites rather than one complete paper. This tendency, widespread in some of the well funded disciplines, seriously distorts the issue of productivity of science and deflects the question of sufficiency of resources available for the scientific enterprise. It is my observation that the cost of remaining at the forefront in a field has risen faster than the rate of inflation as defined by the CPI. Perhaps, a serious study should be undertaken to elucidate the inherent inflation rates for carrying out research in different fields where the progress is not defined in terms of number of published papers but by recognizable knowledge and value creation.

Again as a customer of the products resulting from the national support of science, industry relies on the academic institutions to provide the needed scientific and technical human resources. Thus, an additional parameter for priority setting has to be the quality and the cost-effectiveness with which the federally funded science produces future scientists and engineers. Close technical interactions between industry and the academic institutions help foster an environment where there is a continuous interplay between what universities produce and what industries need. Nowhere is this interplay been more effective than in the area of materials and processing. The clear message from the employers to the educators about the changing needs of industry has reshaped education and research directions at universities. But this kind of active participation can occur only in those fields where there is a significant overlap of industry needs with overwhelming interest of the field. For example, we see very little interaction between experimental high energy particle physics research and research in the area of polymeric materials. We should be able to use this yardstick for cross-cutting priority setting among megaprojects.

Let me now turn briefly to the specific issue of Advanced Materials Processing Program. It is gratifying to note that materials synthesis and materials processing activities are beginning to be recognized as being important. As a result of many years of emphasis on analytic capability for analyzing materials, we have an absolutely first rate capability in the academic community for educating and training superb physical chemists and materials analysts. However, the years of benign neglect and often active down playing of the importance of new materials synthesis have taken its toll. Materials synthesis activities, until recently, have been relegated to a second class status. Very few of the brightest budding materials scientists and chemists consider materials synthesis worthy of their talents. Thus, in the area of materials and processing sciences, we need to give higher priority to synthesis of new materials and exploration of new processes for modifying materials. The federal funding agencies and NSF should move away from the overwhelming support in the area of materials analytical activities to materials synthesis. New phenomena and new device concepts will require materials which do not exist. Without these new materials, the capability of the U.S. to capitalize on its scientific advances in the area new phenomenology will be seriously compromised.

If we believe that priority setting is a crucial issue because of limited resources and unlimited opportunities, then it follows that knowledge creation through megaprojects be compared against knowledge creation through small science, both in its inherent importance to the nation and its cost effectiveness. Support by a large political constituency, for example, is a poor mechanism for priority setting in absence of the above criteria. NAE President Bob White has observed that we should focus our attention on how best we can serve national needs. I assert that that support of science should be viewed in the context of satisfying the long term technological needs of the nation. If a project does not pass this filter, it should be given a lower ranking than one that does. When knowledge creation turns out to be the principal driver for supporting a project, perhaps we should also ask the question about the cost incurred by delaying the funding of such a project.

Finally, I would like to comment on the plethora of different, and often internally inconsistent, criteria proposed for priority setting in science on the national scene. We have found that a small number of well-enunciated and well-characterized criteria serve better in the difficult task of priority setting than a large number of ill-defined and sometimes politically motivated criteria. I recognize, though, that task of setting priorities is likely to be easier in the industry than in a politically charged arena. Nonetheless, a doctrine of fairness and an adherence to the important principles which have made science a successful enterprise in national prosperity would go a long way towards simplifying the priority setting process.

Having armed ourselves with plausible set of criteria we are moving forward in the priority setting exercise. However, the perception of the difficulty faced by young investigators in academic science is seriously jeopardizing the long term health of the entire scientific enterprise. The situation of constrained level of funding for science would likely change as the national economy improves, but the diminished expectations of the current generation of young faculty members could be detrimental to the health of the U.S. economy. Perhaps we need to move more rapidly and with more decisiveness to set cross cutting priorities not only among the megaprojects but also between megaprojects and small science which is the principal supplier of new knowledge and scientifically trained manpower needed by the industries.

Mr. Chairman and Members of the Subcommittee, I strongly believe that priority setting in science funding, among the megaprojects in a cross-cutting manner, and among the small science projects within a given field, is very crucial for deriving the maximum benefit from the available resources. Not everything that can be done needs to be done. And not everything that needs to be done deserves to be done now. Only by setting overall priorities can the nation do the right thing at the right time.

C. KUMAR N. PATEL - BIOGRAPHY

C. Kumar N. Patel is Executive Director, Research, Materials Science, Engineering and Academic Affairs Division at AT&T Bell Laboratories, Murray Hill, New Jersey. He joined Bell Laboratories in 1961 where he began his career by carrying out research in the field of gas lasers. He has made numerous seminal contributions in several fields, including gas lasers, nonlinear optics, molecular spectroscopy, pollution detection, and laser surgery.

His discovery of the laser action on the vibrational-rotational transitions of carbon dioxide in 1963, and his invention of efficient vibrational energy transfer between molecules in 1964 led to his experiments which demonstrated that the carbon dioxide laser was capable of very high cw and pulsed power output at very high conversion efficiencies. The carbon dioxide lasers have now become work horses in at least four major fields of applications of lasers. These are: (1) Industrial applications which include cutting, drilling, and welding; (2) Scientific applications which include spectroscopy, nonlinear optics, and optical pumping to create newer lasers such as far infrared lasers and x-ray lasers; (3) Medical applications which include laser surgery in the areas of otolaryngology, gynecology, tumor removal, and general surgery; and (4) Remote probing applications which, among others, include pollution detection, ranging and Doppler radar, as well as a multitude of military uses. No other laser has made a greater impact on the society than the carbon dioxide laser.

For his seminal contributions to high power lasers and quantum electronics Dr. Patel has received numerous honors. These include the Lomb Medal of the Optical Society of America (OSA) (1966); the Franklin Institute's Ballantine Medal (1968); Coblentz Society's Coblentz Prize (1974); the Association of Indians in America's Honor Award (1975); the IEEE's Lamme Medal (1976); NAE's Zworykin Award (1976); TI Foundation's Founders Prize (1978); the OSA's Townes Medal (1982); the Society of Applied Spectroscopy's N. Y. Section Award (1982); the Schawlow Award of the Laser Institute of America (1984); the New Jersey Governor's Thomas Alva Edison Science Award (1987); the Pake Prize of the American Physical Society (1988); the Medal of Honor of the IEEE (1989); and the Ives Medal of the OSA (1989).

Dr. Patel is a member of the National Academy of Sciences and the National Academy of Engineering. He is a Foreign Fellow of the Indian National Science Academy and The Institution of Electronics and Telecommunication Engineers (INDIA). He is an Associate Fellow of the Third World Academy of Sciences. He is a fellow of the Institute of Electrical and Electronic Engineers, the American Physical Society, the Optical Society of America, the American Academy of Arts and Sciences, Laser Institute of America, and the Association for Advancement of Arts and Sciences. Dr. Patel was elected an Honorary Member of the Gynecologic Laser Surgery Society in 1980, and of the American Society for Laser Medicine and Surgery in 1985.

Dr. Patel has served on the NAS Council (1988-1991), its Executive Committee (1990-1991), and the NRC Governing Board (1990-1991). He has served the American Physical Society as a member of the Council (1987-1991) and the Executive Committee (1987-1990). He co-chaired (with N. Bloembergen) the American Physical Society Study of the Science and Technology of Directed Energy Weapons.

From 1979 to 1988 he served as a member of the Board of Trustees of the Aerospace Corporation, El Segundo, California. In January 1986, he was elected to the Board of Directors of the Newport Corporation, Fountain Valley, California. In October 1990, he was elected to the Board of Directors of the California Micro Devices Corp, Milpitas, California.

Dr Patel received B.E. in Telecommunications from the College of Engineering in Poona, India in 1958. He received M.S. and Ph.D. in Electrical Engineering from Stanford University in 1959 and 1961, respectively. In 1988 he was awarded honorary Doctor of Science degree from the New Jersey Institute of Technology.

Dr. Patel is married to former Shela Dixit. They have two daughters, Neela and Meena. Dr. Patel's hobbies include cooking, tennis, and windsurfing.

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Mr. BOUCHER. Thank you, Dr. Patel. Dr. Baker, we'll be pleased to hear from you.

Dr. BAKER. Chairman Boucher, Congressman Packard, thank you very much for this opportunity to address the subcommittee.

My work in oceanography has given me a lot of experience in the general issues of trying to set priorities, and I think we're seeing something new in the process today—that is, an overlay of the practical application of science. I don't think this is a bad thing. In fact, I think it's essential if we're going to be able to achieve the best possible science and technology that we need.

But we have to be sure that, as we do this, that we ensure the development of the best basic science and technology, because in the long run, the way in which we meet the Federal deficit is to have a very healthy economy and I think that requires a strong science and technical base.

One of the places where the FCCSET process has been very effective is the U.S. Global Change Research Program. That program was conceived by two different groups of people—one, the scientists, who saw a scientific need, and the other, the administration and the Congress and the Government managers who felt that it was an important new program that should take place. It is now working well, I think.

It's coordinated through the FCCSET process, and there are three reasons for that. One is, the agencies themselves had strong science-based programs which had been developed, that could serve as a basis for that effort. The second point, and perhaps the most important point, was that the managers of those programs at NSF and NASA, Department of Energy, NOAA, were all people that were willing and able to cooperate in the system. It doesn't always happen, but the right people were there. And then the third point was that a practical need was recognized by the public, by the administration, and most importantly, by Congress. It was the right people combined with the public perception of need.

The FCCSET process led to the establishment of the Committee on Earth Sciences, which is now the Committee on Earth and Environmental Sciences, and in my view, this is one of the most effective interagency committees that has been established. The group got together and established their priorities in strict accordance with the scientific priorities that had been set by the international scientific community. I know there were many who said when this process first started that it would be impossible for an interagency committee to actually publish a document that had priorities listed in priority order, but in fact, each of the documents that's been published as a cross-cut document has, in fact, listed those priorities.

The Global Change Research Program has not just been a research program; it's had an important impact on policy issues. For example, earlier this year President Bush decided to change the phaseout time for chlorofluorocarbons, changed it back five years, because of the results of the Global Change Research Program. That is, the technology that had been developed, the upper atmosphere research satellite that's monitoring ozone, showed a problem in the ozone depletion in mid-latitudes. There was a direct impact of that information on making this decision to phase out the CFC's earlier. An important aspect of that was that industry was brought in from the beginning, they were part of the process, and have been a willing player. I believe that this process has been effective.

Another piece of the activity there that has been very good is the review process. The global change program has received extensive review from the outside community, primarily through the National Research Council, and I think the reviews have been an important part of keeping that program a strong program.

I don't think the process has been completely successful. Some of the original agencies involved have not chosen to be full participants. There are some examples where lower priority programs are funded at the expense of higher priority programs. But I think this happens in all programs, and I think it's only happened to a minor extent in this overall global change program.

I think the administration should be commended for this success. We all know how hard it is to get agencies to work together. And one can look back at the FCCSET process and you can see that it really did not work very well up until the time of the global change program. I think that the combination of things that I mentioned is one of the reasons that that worked.

But I think we need to look at the FCCSET process and ask some questions. Who decides what FCCSET should take on, for example? Right now it chooses to take on those issues that it chooses to take on, but, in fact, is there a broader way of setting the context for FCCSET? FCCSET, in my view, could be the group that Dr. Healy mentioned earlier this morning, where those responsible for programs actually get together. I don't think that happens in the current process. If, in fact, FCCSET were given the mandate to set priorities, I think the first thing you'd see would be that the science policy managers came to the table, and they would have that kind of table-thumping discussion that Dr. Healy mentioned. So I think the process is there but it needs to actually be made to work.

In terms of setting priorities, I would say that, with some limited exceptions—astronomy is a good one—the research community has not been effective at setting priorities across disciplines. This has been a very difficult thing for the community. We've seen it to a limited extent in some of the boards of the Academy, but in my view, this can't really be done by groups of individual scientists. I think it has to be addressed by groups like the Office of Science Technology Policy or by FCCSET. I believe that when you set priorities for the Nation, you don't want just groups of scientists doing that, but the public must be involved.

The role of the National Science Board is an interesting question, and it seems to me that there's an option there that might work, although I think you could hear from the testimony this morning that something like that could probably only work if it were done together with the National Institutes of Health, which is the other major funder of science. I think the FCCSET process is the way to look forward there.

The thing that I see lacking from the current process really is not so much in the administration but in the Congress. I think one of the things we've seen in the Global Change Research Program is the fact that different agencies report to different committees, and it's not clear to me, as an outsider in the process, where there's an overall view in Congress on these cross-cutting programs. For example, we've seen NASA and the National Science Foundation do well in their budgetary requests, NOAA not doing so well, and as a consequence, the overall program suffers. It seems to me that some kind of overall congressional oversight process might be something that could be very helpful.

Thank you very much.

[The prepared statement of D. James Baker follows:]

TESTIMONY TO THE

HEARING ON PRIORITY SETTING IN SCIENCE

SUBCOMMITTEE ON SCIENCE
OF THE
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
OF THE UNITED STATES HOUSE OF REPRESENTATIVES

BY

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INTRODUCTION

Mr. Chairman, thank you for the opportunity to address the Subcommittee on Science. I am D. James Baker, President of Joint Oceanographic Institutions Incorporated, a non–profit research management organization that plans and operates large research programs for the oceanographic community. Today I speak as an individual who has served as a member of several national and international committees related to global change: the National Research Council's Ocean Studies Board, Space Studies Board, Committee on Climate Research, Committee on Global Change Research, and the Committee on Environmental Research. Internationally, I have served as an officer of the Joint Scientific Committee for the World Climate Research Program sponsored by the International Council of Scientific Unions and the World Meteorological Organization and as chairman of the Committee on Ocean Processes and Climate of the Intergovernmental Oceanographic Commission of UNESCO. My work on these committees has given me direct experience with priority–setting both within and across disciplines and I am pleased to share my views on this process with the Subcommittee members.

In general, I believe that priority–setting is an ongoing process in science through a combination of peer review and funding. The scientific community is continually reviewing itself to ensure the survival of excellence; limited funding means that not even all the good proposals will survive. Today, as science finds more and more practical application, there is a new overlay: the priority of useful and applicable science as opposed to more basic research. I don't fault that overlay; in fact, I believe that it is essential if we are to achieve the best application of the science and technology that we develop. But we must ensure at the same time that the basic science we will need in the long term as an underpinning of understanding is adequately funded and that human resources are also adequate.

THE US GLOBAL CHANGE RESEARCH PROGRAM

The US Global Change Research Program is a case in point. The program was conceived by scientists and government managers as a way to support a combination of priority activities: to assess the state of the environment and to understand it well enough to predict with sufficient accuracy those changes that affect society. The assessment is a key part of this process: adequate understanding requires constant monitoring of what is changing and how.

The Global Change Research Program is coordinated by the White House Office of Science and Technology Policy through the interagency FCCSET, with strong encouragement from OMB. In my view, this process has worked very well for three reasons: (1) Several agencies already had strong, scientifically–based programs that could serve as a base for an interagency effort; (2) The managers in charge of those programs were willing and able to cooperate; and (3) The practical need for such an integrated program was clear to the public, the Congress, and the Administration. The right people combined with a public perception of need is a strong mixture.

The FCCSET process led to the establishment of the Committee on Earth Sciences (CES), now the Committee on Earth and Environmental Sciences (CEES). This Committee has proved to be one of the most effective interagency committees ever established. By working together, the agencies found that they could develop a new Presidential Initiative on Global Change Research in line with identified scientific priorities. The priorities have been developed in strict accordance with those developed by committees and conferences of the international scientific community. There were many who said that an interagency committee could not set priorities; but the first report of the CES did just that and has continued to do so.

The Global Change Research Program has had important impacts on policy decisions. Earlier this year, President Bush decided in a period of only a few days to change the phase-out deadline for chlorofluorocarbons from the year 2000 to 1995; a major shortening of the time. He did this because of results from the US Global Change Research Program. One of the major technology efforts of the Program, NASA's Upper Atmosphere Research Satellite, had revealed a hole in the ozone in mid-latitudes. This new discovery showed that drastic measures were required to preserve the ozone layer; Bush's decision reflected that urgency. A important aspect of the CFC decision was that industry had been made part of the process from the beginning, and hence did not object to the decision.

Another proof of the success of this process is the growing role of the Department of Energy in the Global Change Research Program. Using the priority and review process of the Committee on Earth and Environmental Science, the Department of Energy was able to develop, and strengthen under review, a new effort. This effort, the Atmospheric Radiation Measurement program, fills a gap in the previous program and promises important new information on climate change.

In my view, the FCCSET process has been very effective in setting priorities in global change research, and it is clear from the documentation that the priorities represent the consensus viewpoint of the international scientific community. The various committees and reviews by the National Research Council have supported that view. A final point of success in the global change program is that of review. To date, the USGCRP is the only cross-cut program that is subject to rigorous review; it has benefitted from this review process which has been undertaken by the National Research Council. It is important that the results of these reviews be kept in mind as the program proceeds, particularly the aspects of a balanced program of space measurements and surface-based measurements and issues related to data systems. Moreover, the process has not been entirely successful: some of the original agencies involved have not chosen to be full participants, and there are some instances where lower priority programs will get funded for reasons other than scientific priority. But this happens in all programs, and is a very minor part of the overall program.

I think that the Administration should be commended for this success; we all know how hard it is to get agencies to work together. The FCCSET process as applied to global change research can and should serve as a model for other programs.

EXISTING PROCESS OF SETTING PRIORITIES

The existing process varies in its effectiveness over the various groups involved. The research community, as noted above, sets priorities primarily through the peer review process. It has priorities set for it by the fact of limited funding. Limited funding leads to sequencing of programs; generally agreement can be reached about a sequence provided that each set of proponents believes that their program will eventually be implemented. NSF and NASA have each done well in the sequencing process.

But the research community as a whole is not effective at setting priorities across disciplines. This could be a role for the National Research Council, but the NRC has not yet taken on this activity in a major way. Some of the Boards and Committees have addressed the priority–setting process; in particular, the Space Studies Board and the Astronomy and Astrophysics Survey Committee are good examples of how the cross–disciplinary process can work. In my view, the priority setting process across disciplines is one that is best addressed by the Office of Science and Technology Policy. OSTP has been effective in working with the scientific community and federal agencies in developing priorities. It has been able to do so because of the strong leadership of Dr. Allan Bromley.

The role of the National Science Board is an interesting question. In my view there is no question that the NSB, made up of the distinguished individuals that it has, could be a major force in U.S. and international science policy. To date, the NSB has chosen not to take on such a role, preferring to work in the more confined context of programs of the National Science Foundation, which is to be understood, since that is its official charge. However, given the major and central role of the NSF in basic science, there is no reason why it could not take on a broader role. I would expect that it would have to do this jointly with the appropriate advisory and governing bodies of the National Institutes of Health, the other major organization responsible for a major part of U.S. science.

SUGGESTIONS FOR THE FUTURE

What is lacking from the current process? In the arena of global change research, the Administration has been able to build a reasonably coherent program that is consistent with international science priorities. Agencies have been given roles which have little overlap. This is cost–effective, but only works if all agencies are funded adequately for their share of the program. It appears to me, as an outsider in the process, that it has been difficult for Congress to respond to this coherent program because different agencies report to different committees. For example, NOAA, which has an important part of the overall program, reports to a different set of committees from NASA or NSF. With no general oversight, there is the danger that one agency's global change research program will suffer for reasons not related to the program, and as a consequence, important aspects of the overall program will not be done.

I believe that one of the most important things that could be done at this point would be for Congress to establish an oversight process, perhaps a caucus process, that would provide the necessary information to all Congressional Committees, authorizing and appropriations, that act on the global change research program.

Thank you very much for the opportunity to present my views on these topics to the Subcommittee. I would be pleased to answer any questions.

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Mr. BOUCHER. Thank you very much, Dr. Baker and Dr. Patel.

Let me begin by responding to your last comment, Dr. Baker. To the extent that there is a place where an overall view is taken, you're there. It's here. We have the charter in this subcommittee of general oversight of Federal science policy. One of the unfortunate aspects—and you've alluded to this as well—is that the jurisdiction for making authorizing and appropriating decisions is not centralized, and while we have a general oversight function, our actual authorizing jurisdiction is more or less limited to the programs of the National Science Foundation.

The Energy and Commerce Committee authorizes the programs for the National Institutes of Health. The Committee on Agriculture has a role with regard to the Department of Agriculture and its research. The Armed Services Committee has a role with respect to DARPA and what is done there. So you're quite right in pointing to the fact that there is a fractionalization of responsibility here in the Congress with regard to making these decisions.

That's why I think many of us are of the view that the more appropriate place to begin this process is really not internally with the Congress but in the executive branch. I think your recommendations here, as a panel testifying on the FCCSET process, are particularly helpful in terms of informing us as to ways that the executive branch can perform that function.

I noticed that you, Dr. Baker, are inclined to agree that FCCSET is the place where that measure of cross-cutting should take place. I noticed also your allusion to Dr. Healy's comments earlier this morning that perhaps the agency heads themselves should have a stronger role to play—and we've noted that as well.

Tell me what recommendations you have to this subcommittee of a very specific nature for ways that, legislatively, we could enhance that FCCSET process? What needs to be done better to institutionalize their cross-cutting decisions that really would have the effect of setting priorities for Federal funding of science?

Dr. BAKER. I think it's a very straightforward answer to that question. The Federal interagency committee's work, if the committee has a charter to do something important, like set a budget, or if it's told by OMB that it has to get together and come up with a coordinated plan—one of the reasons we have a Global Change Research Program is not all the reasons that I mentioned but also because OMB had a strong interest in it. If, in fact, FCCSET were given the charge to set priorities—and maybe that's something that could be pushed by Congress—then, in fact, the agency heads would come to the meetings and they would be involved, because they would see it as an important committee. If they see the committee as just another interagency committee, where everybody lays their plans on the table, they're all blessed and then they go home, a lower and lower level person from the agencies is sent each time and so the committee becomes ineffective. And that's really what happened to FCCSET in previous years.

I think we're seeing a change in that now, as they have more visibility. But I think pushing FCCSET to have a stronger responsibility, and possibly closer to budget priorities, I think is one way to get the agency heads to sit down. If they don't have the authority, then no committee will work, no matter who the designee is.

Mr. BOUCHER. So we should do something to enhance the dignity of FCCSET, give it a stronger role in the process, and then you think the agencies necessarily will become more active players in the FCCSET deliberations.

Dr. BAKER. They'll come where the action is.

Mr. BOUCHER. Dr. Patel, would you care to comment?

Dr. PATEL. Yes. I think the important issue here is, as Dr. Baker pointed out, giving the responsibility and accountability for setting priorities, but in a sense, that charge clearly has to come from the Congress within the framework of the question of how much science is necessary to assure the future prosperity of this country. I think to have a priority setting body in the absence of an overall goal, vision for the society, doesn't get us too far. What is required here is not only a set of priorities but the amount of resources that one should devote to science because it meets with some overall vision for the society.

But on the whole, I think if the committee believes it is going to do something worthwhile, and is going to be held accountable for it, I think one can increase its visibility and the quality of work that is produced.

Mr. BOUCHER. In terms of trying to achieve that vision, which I think you correctly suggest we have to have, what about combining several of the recommendations we've heard here today, starting with an upgrade for the dignity of the FCCSET process and some direction that it actually perform a cross-cutting function in terms of priority setting, combined with a direction that FCCSET, in turn, call on the various disciplines for recommendations of research priorities within those disciplines.

The vision that we would get, presumably, then would come from the disciplines themselves. The researchers who have hands on responsibility and direct knowledge of what is possible and what opportunities are would be called on to make recommendations with regard to those various measures to the FCCSET committee. Then the FCCSET committee, using that information and that knowledge, could make cross-cutting decisions with respect to the various projects that are recommended.

What would you think about a structure like that? How effective could that be? What kinds of problems do you think would arise if we were to put that in place?

Dr. PATEL. Clearly, the priority setting process has to be done at many levels. Initially, the priority setting in a given discipline has to take place, and then it has to be escalated. But I think one of the important features that is missing in such a scenario is again coming back to the initial question of for whom is this work being done? If the science is being done for other scientists, clearly the scientists themselves are appropriate—is the appropriate body to make those decisions.

But I, for one, don't believe that all science is done for the sake of science. Science is being done because it fulfills some longer range goal for the society. It's important to bring in that constituency into the FCCSET decision making process so that we don't have a priority setting process that is totally separated from where the society would like to end up five or ten or fifteen years from now.

Mr. BOUCHER. So the users of technology need to be involved in some way in those discussions.

Dr. PATEL. I certainly believe so. I think certainly for physical and materials sciences, it's absolutely essential, and I think as we saw this morning, for health sciences, the close relationship with the state of the society's health is also very important.

Mr. BOUCHER. Let me challenge you on one statement you made earlier. You suggested that the decisions with regard to funding basic science should be driven by the extent to which success in those various projects would create usable technology. Now, I grant you that that's one of the primary purposes of our basic science research program. We are trying to create usable technology.

But you didn't leave room in your statement for another goal that I think is valid, and that is the creation of knowledge. Do you not agree that that is also a valid objective of basic science research, and if so, would we not be doing a disservice if we allowed the creation of technology to drive all of our decisions in terms of what projects are funded and which are not?

Dr. PATEL. Let me amplify my observation there. I pointed out that both knowledge creation as well as value creation are important. Then it only comes down to the question of how long is the pipeline, the pipeline at the initial stage, where you create new knowledge at the time when it is being used. For some fields, that pipeline is a lot longer than for other fields, and I submit to you that, for example, for high energy particle physics, that pipeline is exceedingly long. Some of those findings will eventually become part of our understanding of low energy physics, which then will become part of the technology that one will develop.

So it is not a matter of this or that, but asking the question how long is the pipeline, and which long pipelines are better than other long pipelines, which short pipelines are better than other short pipelines, and over what kind of time scale can we afford to do what.

The point I made, namely, specifically about not everything that needs to be done needs to be done now, is applicable here. Places where the pipelines are exceedingly long, one can wait; places where the pipelines are short, where the payback period is ten years, one may want to move faster.

Mr. BOUCHER. Well, should that always be the deciding factor, the length of time that it takes to produce the result? Does that not rule out long-term and somewhat uncertain kinds of research?

Dr. PATEL. Mr. Chairman, I submit to you that it does not, because the key point is not that we should not do any of the very, very long-term projects; the question is they have to be put into proper perspective, in terms of how long it will take for us to get there. The key point is, if you don't survive, if the economy of this country does not survive for the next ten years, anything that we do right now that might help us 30 years from now may be of no value. So just as we run our personal lives, in a manner which has sort of a degree of decision making, we need the same kind of decision making also on a national scale.

Mr. BOUCHER. I want to thank both of the witnesses for their responses. I have some other questions which perhaps we'll ask of you another day.

The gentleman from California, Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman.

Let me pursue just a little further, Dr. Patel, the line of questioning that the Chairman was asking.

Does the private sector—and you, I think I observed, may be the only representative from the private sector in our hearing today, from AT&T Bell Labs. From the private sector's point of view, do they have their own set of criteria, that they develop their own priorities, and if so, are they somewhat driven by Federal and agency priorities, or are they independent, on a separate track that is not coordinated?

Dr. PATEL. The priority setting process we have at AT&T Bell Laboratories—and that's the only one I can really comment on—is driven by two principal mechanisms. As it applies to conceptually new science, it is driven by opportunities, scientific opportunities, intellectual opportunities, which are available. Those, to some extent, are closely tied with the national science scene.

The second part, which is what I mentioned, the maintenance research, is driven by our internal priorities, where we expect the company's technology needs would be five years from now or ten years from now, and try to fashion our research in some broad way to pursue that kind of category.

The point I had made earlier about the research on high temperature superconductors being one of these conceptually new research activity, we have a substantial amount of activity in high temperature superconductors at Bell Laboratories, although it is not connected with any of our near-term or even the long-term company needs. But we believe that the understanding of materials that we will gather from that will have an important bearing on how we will do our business five or ten or twenty years from now.

Mr. PACKARD. Is there any great coordinating or correlation between the prioritizing at Federal agency levels with the private sector, or is the private sector invited or involved in assisting in the Federal priority setting for science?

Dr. PATEL. To the best of my knowledge, this does not happen as a matter of course. I'm sure there are exceptions, but—

Mr. PACKARD. Structurally, you're not aware of anything that—

Dr. PATEL. Structurally, I'm not aware of any process that allows the technological needs of the country to be fed back into the science priority setting process.

Mr. PACKARD. In your judgment, is that a weakness of our system?

Dr. PATEL. I certainly believe that is a weakness, not because industry wants the Nation to do the research that it needs, but it wants to make sure that, if there are any holes, that the proper priority setting bodies be aware of those.

Mr. PACKARD. Thank you very much.

Dr. Baker, one question. The FCCSET is an organization that is essentially designed to pull together the agency leaders, the heads of agencies, in an effort to coordinate prioritizing and determining where science ought to go. Is there a need to pull in other scientific people, the science community, academia and others, into that process, or are they already involved in that process, or do we leave that to the agency leaders and heads to address that need or that area of input?

Dr. BAKER. This has been an ongoing question in the Global Change Research Program, because the FCCSET members of the subcommittee, the Committee on Earth and Environmental Sciences, are agency heads, Government program managers. The question is how to feed in the rest of the scientific community, the academic community, into that.

There has been a very close relation between that committee and the various committees of the National Academy of Sciences, the Committee on Global Change Research, for example. I think that what we found is that the process works to some extent—that is, there's a tendency for the Government committee to meet by itself; there's a tendency for the academic groups to meet by themselves; and they don't tend to naturally come together. We've had some forced interactions which I think have shown that there is a bit of a problem.

The Academy has restructured its global change committee into a higher level committee. It's now called a board. I think it will have a broader representation, with the strict point that it wants to have a closer relation with the activities of the Committee on Earth and Environmental Sciences. I think there's a feeling from the FCCSET side that that's important, also.

So I think you've put your finger on an important point, which I think there is an attempt now to address it and we'll see how it works.

Mr. PACKARD. Mr. Chairman, I think we've had an excellent hearing, and I think our witnesses have been particularly valuable with their input, and this panel certainly is included in that. I want to thank you very much.

Mr. BOUCHER. Thank you, Mr. Packard. I share your view of this hearing. It has been, I think, very informative. I particularly thank this panel for suggestions and recommendations that it has made.

I have one additional question, Mr. Baker, of you. We have received some information recently that Mr. Bromley is planning, to use his words, "to mainstream the Global Climate Change Initiative". Do you have any knowledge of what is being proposed and what possible effect that will have in terms of creating a change in the way that that program is currently administered?

Dr. BAKER. This is an interesting point. I think the FCCSET process, as it currently is arranged, they feel they can only handle a few presidential initiatives at a time—four or five. They see many needs for these presidential initiatives which have the characteristics of being very important to do and needing a fast spin- up on a budget. So what they have looked for is a way of moving what had been presidential initiatives—the Global Change Research Program and the high performance computing initiative are examples—into what they would call—I think the word is a "national research program"; I don't know the exact terms. I'm fully aware of that fact.

I think one has to make sure that the term "national research program" is not a code word for lower priority and less funding in the future, which is always a possibility, but that these programs which have been deemed to be of high priority, in fact, continue to get the kind of funding and support that they have. I think that's something we have to watch.

It is not clear to me—In the defense of the process, it's not clear to me that one can continue to have new initiatives existing for many years. I think the initiative has to become a mature initiative and—it has to become a mature program is probably a better way to put it. So our Government has to find a way to do that.

I think the real question is, why can FCCSET only handle a few initiatives at a time? It seems to me that this body ought to be broad enough and have a broad enough context so it really could handle all of these important programs for us.

Mr. BOUCHER. The notion here being, I suppose, that there is a proposed mainstreaming—and I'm going to ask you if you know what that term means. But there's a proposed mainstreaming of the Global Change Research Program because FCCSET is becoming overburdened with these interagency initiatives? Is that the idea?

Dr. BAKER. I think that probably cuts to the heart of it.

Mr. BOUCHER. What does "mainstreaming" really mean?

Dr. BAKER. As I understand it, it means that the global change program would no longer be a presidential initiative. I could be wrong on the details here, but I think that's approximately right. It turns from being a presidential initiative to what's called a national research program. The national research program, I don't think we've had one of these in the past, so it's striking new ground. But I think it would still have high priority. It probably would have a budget run up which is somewhat less than the program had at the beginning, all of which is not unreasonable considering how we try to fund programs, and that it would have high enough priority so that its budget level, as it went up to its expected level, that budget level was sustained over the period of the program.

The Global Change Research Program is envisaged to be a 20 to 30 year program, and so one wants to see adequate support for that. But I think that's what they mean when they say "mainstream".

Mr. BOUCHER. Are you concerned that to mainstream this program might lessen its significance and therefore result in decreased levels of funding?

Dr. BAKER. That's a concern. I'm always concerned when a program I'm interested in is losing a high priority status, so I guess the research community and those who are interested will be out there trying to make sure that it does have adequate support.

Mr. BOUCHER. Well, thank you for those responses. We'll be posing those questions to others over the coming weeks.

Gentlemen, we thank you again for your attendance today. This subcommittee will hold two additional hearings on the subject of priority setting for Federal research funding later this month and during the month of May.

There being no further business to come before the subcommittee today, this hearing is adjourned.

[Whereupon, at 1:30 p.m., the subcommittee adjourned.]

SETTING PRIORITIES IN SCIENCE

TUESDAY, APRIL 28, 1992

House of Representatives,
Committee on Science, Space, and Technology,
Subcommittee on Science,
Washington, D.C.

The subcommittee met, pursuant to call, at 9:36 a.m., in room 2325, Rayburn House Office Building, Hon. Rick Boucher [chairman of the subcommittee] presiding.

Mr. Boucher. The subcommittee will come to order.

This morning, the Subcommittee on Science holds its second hearing on the need for setting priorities in Federal research funding. Priority setting is a complex process requiring a unique balance and integration of advice from the research community, Executive Branch agencies, and the Congress. Our goal in examining the priority setting process is to ensure that the most promising research continues to be funded, that the Federal Government supports a full and balanced research portfolio, and that research results contribute to national objectives.

The success of America's research system has spawned rapid growth in both the academic research enterprise and in research opportunities. Real growth in Federal funding for research has not kept pace with this expansion. Today there are more good research ideas than can be funded, more researchers than can be supported, and more requests for Federal support of research than can be met. There is now a clear need to establish a priority setting process that will ensure that Federal investments in research are in furtherance of well defined national goals.

At our hearing on April 7, the subcommittee was encouraged to explore mechanisms to motivate the research community to establish research priorities within each given discipline and to recommend research priorities across disciplines. The subcommittee was also alerted to the need for Federal agencies to establish strategic and long-range plans and priorities that go beyond the annual budget process. To facilitate the development of prudent Federal research priorities, witnesses stressed the need to require the responsible officials in each agency to meet on a regular basis to integrate that agency's research priorities into a coordinated set of national priorities.

The hearing today will consider these and other recommendations that have been made to the subcommittee for improving the Federal priority setting process.

We have a distinguished panel of witnesses this morning, and the subcommittee extends its welcome to each of them.

Before turning to you for your testimony, I would like to now recognize the ranking Republican member of the subcommittee, the gentleman from California, Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman, and I too wish to welcome the witnesses, all witnesses today but particularly now the first panel—we appreciate you being here.

The first hearing in this series, which was held on April 7, set off a candid discussion on how Federal agencies go about setting priorities for the research projects they fund. We also looked at specific examples of priority setting by two research communities and by the Federal Coordinating Council on Science, Engineering, and Technology.

Although it may be possible to encourage a system of priority setting within various scientific fields and within Federal agencies, the real challenge lies in setting priorities among different fields of research and across Federal agencies.

As the Federal budget becomes increasingly a zero sum game, it becomes even more critical that we establish priorities for Federally funded research. Accordingly, therefore, I do look forward to the testimony today and the witnesses that have taken time out of their busy schedules. I want to express appreciation and look forward to a discussion on this very important issue.

Thank you, Mr. Chairman.

Mr. BOUCHER. The chair thanks the gentleman.

We welcome now our panel of witnesses for today: the former Governor of Ohio and Chairman of the Government-University-Industry Research Roundtable, the Honorable Richard F. Celeste; Dr. Harvey Brooks of the John F. Kennedy School of Government at Harvard University; Dr. Ralph Gomory, President of the Alfred Sloan Foundation and former Senior Vice President for Research at IBM; and Dr. John Dutton, Dean of the College of Earth and Material Sciences, Pennsylvania State University, and Chairman of the Task Group on Priorities in Science Research for the Space Studies Board.

We will make, without objection, the prepared written statement of each of the witnesses a part of our record and would welcome a summary, an oral summary, of your testimony; and, Governor Celeste, we will begin with you.

[The prepared statements of Messrs. Boucher and Packard follow:]

OPENING STATEMENT
OF THE
HONORABLE RICK BOUCHER (D-VA)
CHAIRMAN, SUBCOMMITTEE ON SCIENCE
ON
PRIORITY SETTING IN SCIENCE

9:30 a.m. - 2325 RHOB
April 28, 1992

This morning the Subcommittee on Science holds its second hearing on the need for setting priorities in federal research funding. Priority setting is a complex process requiring a unique balance and integration of advice from the research community, Executive Branch agencies, and the Congress. Our goal in reviewing the priority-setting process is to ensure that the most promising research continues to be funded, that the Federal Government supports a full and balanced research portfolio, and that research results contribute to national objectives.

The success of America's research system has spawned rapid growth in both the academic research enterprise and in research opportunities. Real growth in federal funding for research has not kept pace with this expansion. Today, there are more good research ideas than can be funded, more researchers than can be supported, and more requests for federal support of research than can be met. As Dr. Patel stated during our April 7 hearing, "Not everything that can be done needs to be done. And not everything that deserves to be done deserves to be done now." The objective is to institutionalize a priority setting process that will ensure that federal investments in research are in furtherance of well defined national goals.

At our April 7 hearing, the Subcommittee was encouraged to explore mechanisms to motivate the research community to establish research priorities within a discipline and to recommend research priorities across disciplines. The Subcommittee was also alerted to the need for federal agencies to establish strategic and long-range plans and priorities that go beyond the annual budget process. To facilitate the development of prudent federal research priorities, witnesses stressed the need to require the responsible officials in each agency to meet on a regular basis to integrate each agency's research priorities into a coordinated set of federal priorities.

This hearing will consider these and other recommendations for improving the priority setting process. Our next hearing will review the specific role of the Office of Science and Technology Policy (OSTP) and the Office of Management and Budget (OMB) in setting priorities for federal research funding.

We are pleased to have with us this morning:

Dr. Harvey Brooks
John F. Kennedy School of Government
Harvard University
Cambridge, MA

Dr. John A. Dutton, Dean
College of Earth and Mineral Sciences
Pennsylvania State University
University Park, PA
and
Chairman, Task Group on Priorities in Space Research,
Space Studies Board

Honorable Richard F. Celeste
Chairman,
Government-University-Industry Research Roundtable
and
former Governor, Ohio
Columbus, OH

Dr. Ralph Gomory
President
Alfred P. Sloan Foundation
New York, NY
and
former Vice-President for Research, IBM

I would like to extend a warm welcome to our witnesses. We appreciate your coming to meet with us this morning and look forward to your testimony.

STATEMENT OF
THE HONORABLE RON PACKARD
SCIENCE SUBCOMMITTEE
SECOND HEARING ON SCIENCE PRIORITIES
9:30 A.M., 2325 RHOB
APRIL 28, 1992

Thank you, Mr. Chairman:

The first hearing in this series, which we held on April 7, set off a candid discussion on how federal agencies go about setting priorities for the research projects they fund. We also looked at specific examples of priority setting by 2 research communities and by the Federal Coordinating Council on Science, Engineering, and Technology.

Although it may be possible to encourage a system of priority setting within various scientific fields and within federal agencies, the real challenge lies in setting priorities among different fields of research and across federal agencies.

As the federal budget becomes increasingly a zero-sum game, it becomes even more critical that we establish priorities for federally funded research. Accordingly, I look forward to the testimony we will hear today and I welcome all the witnesses that have taken time out of their busy schedules to address this very important issue.

STATEMENTS OF HON. RICHARD F. CELESTE, CHAIRMAN, GOVERNMENT-UNIVERSITY-INDUSTRY RESEARCH ROUNDTABLE, AND FORMER GOVERNOR, OHIO, COLUMBUS, OHIO; RALPH GOMORY, PRESIDENT, ALFRED P. SLOAN FOUNDATION, NEW YORK, NEW YORK, AND FORMER VICE PRESIDENT FOR RESEARCH, IBM; HARVEY BROOKS, JOHN F. KENNEDY SCHOOL OF GOVERNMENT, HARVARD UNIVERSITY, CAMBRIDGE, MASSACHUSETTS; AND JOHN A. DUTTON, DEAN, COLLEGE OF EARTH AND MINERAL SCIENCES, PENNSYLVANIA STATE UNIVERSITY, UNIVERSITY PARK, PENNSYLVANIA, AND CHAIRMAN, TASK GROUP ON PRIORITIES IN SPACE RESEARCH, SPACE STUDIES BOARD

Mr. CELESTE. Thank you very much, Mr. Chairman.

Let me indicate that, while I'm a former governor and chairing the Government-University-Industry Research Roundtable, today I offer personal views, for what they are worth, informed a bit by that experience.

I think there are two questions that need to be considered. The one in a sense has been answered by the subcommittee's interest, but: "Why is priority setting so important?" the first question; and then, "Who should be responsible for it?" is the second question.

Science is exploding with research promise. The expectations of science are exploding as well. The research enterprise has grown enormously diffused since the 1960's; there are more than 200 research institutions across the country today. This is no longer just a Federal Government issue, and, as a former governor, I would stress, more and more State governments are involved in making investments in scientific research. The increasing expectations and the high level of investments have raised very tough questions about accountability. And, finally, research agendas of other nations have a growing impact on our own.

So all of these are reasons why, in addition to the scarcity of resources, which is what usually leads policy makers to getting to the priority setting issue. This is not to say that scarcity of resources isn't a compelling reason for priority setting as well; using our resources wisely, leveraging them well, is a critical goal, it seems to me. But I would like to focus for just a few moments on the question of who should be responsible for national priority setting.

At the level of the scientific discipline where many of the decisions affecting the individual investigator are made or influenced, I would begin by observing that there is a deep concern about excessive planning, about too much specificity in setting research priorities, or in setting priorities for fundamental research.

One of the participants at the Roundtable's December conference last fall said "you can't plan discovery", in response to an extended discussion about how do we set priorities for basic research, and I think she spoke for most of the people who were at that conference on the future of the academic research enterprise.

At the level of Federal agencies, real effort has to be devoted to being clear about the agency mission and about the way in which scientific research priorities relate to that mission, and here the issue becomes, how does the science contribute to broader social, economic, and political objectives. In other words, what I would call civic criteria must also be concerned in priority setting, not simply the excellence of the research or the degree to which it might contribute to our knowledge base. Potential investments are going to be ranked for relevance as well as elegance when it comes to how Federal agencies look at research.

So the criteria are broadened, but the participation in decision making about priority setting really isn't broadened significantly. It is at the highest level—namely, across the Federal Government, and, I would argue, even beyond the Federal Government—in setting kind of national priorities, not just Federal priorities, that the administration and the Congress can confront the clearest need for priority setting, and it is at this level that critical issues have to be resolved—cross-cutting agencies, cooperation between the Federal and State level or other participants in research, who is responsible for infrastructure, how do we ensure long-term commitments. I just saw a little thing on Thomas Edison, and it said that every light bulb he invented failed until he reached 8,996. How do we make long-term commitments?

Individual investigators and Federal agency officials have a stake in the decisions at this level, but they can't resolve them. The issues require a mechanism and, I would argue, a process. As I read the reports of the last hearing, a good deal of the focus was on the mechanism, particularly around FCCSET, and there were some suggestions about how its membership might be broadened or complemented, and I certainly—while I don't have a recommendation there, I think that the notion is: How do you get the right Federal players to participate in a timely and sustained way? and I look forward to the committee's wisdom and the Congress's wisdom in that regard.

But I'm concerned about the *process* for priority setting as well as the mechanism. I stress process because I see that as how you reach out to involve a larger number of participants. I think that broad participation is critically important to sound priority setting, for priorities that will be durable, that will be compelling, that will work.

A number of universities have done an excellent job of strategic planning. Their priority-setting exercises have helped them to focus on key centers of excellence, to stimulate cross-department collaboration, to identify ways of leveraging existing resources, and to make tough choices in allocating limited resources. But the lesson is: Where their strategic planning did not engage all of the stakeholders, the priority agenda stimulated resistance rather than cooperation.

We need to invent new processes that tap the creativity of our research community, that join their creative energy with that of policy officials in industry and universities as well as government, and that invite and assure widespread public understanding and support for the research agenda that is developed.

One suggestion for stimulating continuing experimentation with priority setting comes out of the Roundtable's December conference. Two separate discussion groups, one focused on priority setting, one on strategic planning, came to essentially the same conclusion. They said with respect to priorities within a single university or funding agency or Federal lab, the participants concluded these institutions need to establish clear goals prior to setting the priorities and develop better ways to mobilize and allocate resources in order to achieve those goals. Different approaches to priority setting could be considered, and there isn't one right way to do this.

In addition, they thought periodically a national forum should be held where institutions across the spectrum could discuss and compare their plans and priorities. Such a forum, it was felt, would generate beneficial synergism by revealing gaps and overlaps, areas for elimination, and areas for potential cooperation.

With respect to priorities within a discipline, the conference participants concluded strategic plans and priorities of the institutions and agencies should emerge from the existing opportunities across the spectrum of scientific fields. This means that the research community must be part of the process from the beginning, with each community presenting plans and priorities for its discipline and not simply responding to plans that are initiated at an institutional level.

Finally, the participants emphasized that the starting point of priority setting and strategic planning is understanding and stating clearly the objectives and goals of the particular institution or discipline, and in many cases there is a lack of clear mission statements at universities, at Federal labs, and even among the Federal agencies themselves.

The Roundtable, for its part, through a series of focus groups and national colloquia, will attempt to convene the kind of process which I have just described over the next several years so that we can learn from that process. The Roundtable discussion paper, "Fateful Choices," proposes a framework for considering the kind of complex and often subtle issues that are involved in priority setting, and I hope that you, Mr. Chairman, and the members of this subcommittee will have an opportunity to explore that perhaps during your upcoming hearing.

Thanks very much.

[The prepared statement of Mr. Celeste follows:]

SETTING PRIORITIES IN SCIENCE

Statement of

RICHARD F. CELESTE,
CHAIRMAN
THE GOVERNMENT-UNIVERSITY-INDUSTRY RESEARCH ROUNDTABLE

NATIONAL ACADEMY OF SCIENCES
NATIONAL ACADEMY OF ENGINEERING
INSTITUTE OF MEDICINE

BEFORE THE

SUBCOMMITTEE ON SCIENCE
THE COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY

UNITED STATES HOUSE OF REPRESENTATIVES

APRIL 28, 1992

My name is Richard Celeste. I am the former Governor of Ohio. Currently I chair the Government-University-industry-Research Roundtable. Both of these responsibilities have contributed to my thinking, although the views I share with you today are mine alone.

I speak to you today in support of the proposition that national priority setting is vitally important if we are to sustain this Nation's role as the world pacesetter in scientific research. I want to focus on two questions: Why is priority setting so important? Who should be responsible for it?

Why Is Priority Setting So Important?

Consider the decision-making landscape for our research enterprise in the 1950s and 1960s.

--Then there were few research universities. The Seaborg Report of 1960 estimated that there were about 20, and proposed doubling that base to 40 as a national priority.

--Then a small number of scientists and engineers, representing a handful of disciplines and institutions, were recognized as national leaders and spokesmen capable of making disinterested judgments about the needs of the Nation's research enterprise.

--Then policy choices confronting these leaders were tightly focussed because the national political leadership and the research community were coalesced around a common purpose: to enhance U.S. national security through an enlarged research capacity.

Today our research decision making landscape is dramatically different.

--Science is exploding with research promise. We are living in an era of unparalleled discovery. The growing array of new scientific and technological opportunities inexorably push the enterprise toward expansion. The increasing rate of discoveries push us in this direction. So do the exciting advances in scientific and computational instrumentation. So does the globalization of the research enterprise.

--Expectations of science are exploding as well. As the Cold War has ended, an urgent array of public policy issues press for the attention of the research community. Environmental issues, public health concerns, economic competitiveness, fairness and equity all vie for position on the Nation's scientific research agenda.

--The research enterprise has diffused. The academic research enterprise has expanded enormously since 1960 -- today numbering more than 200 research institutions and more than 150,000 investigators. The composition of the research community is changing, though by no means quickly enough. Its new members, especially women and minorities, often bring a fresh set of expectations and critical social values to their work and their work places.

--This is no longer just a Federal Government issue. State governments have become new investors in the research enterprise. An increasing number of states are choosing targeted investments in scientific research to stimulate the local economy or to win national or international recognition. (Remember the response of the State of Utah to the cold fusion announcement!) State governments invested nearly $1 billion in science and technology initiatives in 1990 alone.

--Increasing expectations and high levels of investment are raising tough questions related to accountability. How do we judge whether our public money is well spent? What should we expect for it -- new knowledge or immediate social benefits? How do we avoid unnecessary duplication? How do we ensure scientific integrity?

--Moreover, the research agendas of other nations will have a growing impact on our own. Nations like Japan, Germany, even China, seek their comparative advantage in scientific research, and regional organizations like the European Community set clear cut scientific research priorities.

Each of these six developments suggest not only how far we have come since the relatively uncomplicated days of scientific research priority setting in the 1950s and 1960s. They also suggest why national priority setting is important beyond simply as a response to scarce resources. Though, I would add, scarce resources are another compelling argument for a sound priority setting process.

Let me summarize why priority setting is essential as we move toward the 21st Century. The opportunities for scientific discovery are more abundant than our resources. So are public expectations. So are institutional needs. The potential for collaboration with domestic partners (state governments) and international partners is growing. But, without clear criteria for choosing where to focus our investments in science, opportunities are lost and genuine accountability is a pipe dream. What should be an occasion for vibrant public debate and decision making, instead becomes an excuse for insider griping, squandered resources, uncertain initiatives, and depreciated and discouraged scientific assets and researchers.

Who Should Be Responsible For National Priority Setting?

I commend the Subcommittee on the care taken in the letter of invitation to this hearing to specify the three levels at which priority setting will be considered. One of the central issues related to who is responsible is the level at which the priority setting decisions are being made. (For a helpful discussion of this aspect see "Framework for Assessing Science and Technology Budgets" in the 1988 Academy report, Federal Science and Technology Budget Priorities: New Perspectives and Procedures.)

Let me start at the level of the scientific discipline where many of the decisions affecting the individual investigator are made or influenced. At this level there is, understandably I believe, a deep concern about excessive planning, too much specificity in setting research priorities for fundamental research. As one participant in the Roundtable's December 1991 national conference on "The Future of the Academic Research Enterprise" exclaimed, "You can't plan discovery!"

Yet you can embrace certain criteria and you can determine who ought to participate in choosing among a variety of possible research investments. In the case of priority setting at the level of the individual research project within a discipline or field of research, the criteria have been fundamental: the quality of the proposal (rank excellence at the top) and the potential impact on our knowledge base (rank promise at the top). On the latter score, originality and relevance to civic needs may be considered but are not essential.

These criteria are most frequently deployed by peers who are in most cases the defacto decision makers about research priorities at this level. Thus, this priority setting is done deep within the research community itself. The scientific societies can help to set additional criteria to assist in the decision making process. You heard some useful examples in your April 7th hearing in testimony regarding astronomy and ecology. However, by design participation is relatively narrow which poses a serious challenge in terms of fiscal and social accountability.

One important issue in priority setting at the national level is how much of our resources go to support research at the individual or research team level in disciplines where the funding decisions are largely unencumbered by any higher level expectations or requirements, such as agency mission or national goal. Should one priority be viewed as assuring some absolute amount, or some percentage, of our total Federal investment in scientific research as committed to "basic science" in its most unfettered sense?

At the level of Federal agencies, real effort must be devoted to being clear about the agency mission and the scientific research priorities which support that mission. Here the issue becomes how does the science contribute to broader social, economic and political objectives. In other words, what I would call "civic criteria" must also be considered in priority setting. Potential investments will be ranked for relevance as well as elegance. While criteria are broadened, however, participation in decision making is still relatively narrow.

It is at a higher level still, namely, across the Federal Government (usually in connection with preparing the President's Budget for submission to Congress and in responding to that Budget proposal), that the Administration and the Congress confront the clearest need for priority setting. For it is at this level that critical issues must be resolved, hard choices made.

Opportunities for research which cuts across agency boundaries (as many environmental, public health, energy and economic problems do) must be identified clearly. Opportunities for cooperation with state initiatives or international undertakings must be evaluated. The importance of investing in infrastructure must be determined. The willingness to consider long term commitments must be addressed. (Thomas Edison made more than 8900 light bulbs before one worked!).

While individual investigators and Federal agency officials have a stake in these issues, they cannot be resolved at their levels. The resolution of these issues -- which necessarily involves priority setting (or, failing that, enormous lost opportunity costs, duplication of efforts, ineffective initiatives, and so on) -- requires, in my view, both <u>a mechanism</u> and <u>a process</u>.

A good deal of the discussion at the previous hearing had to do with whether FCCSET is the appropriate mechanism. Clearly the OSTP and OMB are the agencies with the broadest reach on this matter and with the most to contribute. The critical issue, I sense, is whether (and if so, how) to expand or modify FCCSET so that all of the right Federal players participate. I am encouraged by the discussion of this matter which your hearings have stimulated and will be keenly interested in the outcome.

But, I am concerned about the <u>process</u> for priority setting as well as the <u>mechanism</u>. I say this because whatever <u>mechanism</u> is evolved by Federal executive and legislative branches working together, it will necessarily involve a very limited number of participants. And broad participation is essential to sound priority setting.

Some universities, for example, have done an excellent job of strategic planning. Their priority setting exercises have helped them 1) to focus on key centers of excellence; 2) to stimulate cross department collaboration; 3) to identify ways to leverage existing resources; and, 4) to make tough choices in allocating limited resources. But where their strategic planning did not engage all of the stakeholders, the priority agenda stimulated resistance rather than cooperation.

We must invent new processes that tap the creativity of our research community, that join their creative energy with that of policy officials in industry, universities and government, and that invite and assure widespread public understanding and support. We need a process that will help all of us understand what is at the top of our priority list for the national research agenda, and why it's there. We need a sense of how our enormously varied research investments fit together, and what we can expect of them.

One suggestion for stimulating continuing experimentation with priority setting came out of the Roundtable's December conference. Two separate discussion groups -- one on priority setting and one on strategic planning came to the same conclusion:

- With respect to priorities within a single university, funding agency or federal laboratory, the conference participants concluded: Universities, funding agencies, and federal laboratories need to establish clear goals and to develop better ways to mobilize and allocate resources in order to achieve those goals. Different approaches to priority setting should be considered. Universities, agencies and laboratories each should engage in self-examination and generate institution-specific strategic plans. Periodically, a national forum should be held where institutions could discuss and compare their plans and priorities. Such a forum, it was felt, would generate beneficial synergism by revealing gaps and overlaps, areas for elimination, and areas with potential for cooperation.

- With respect to priorities within a discipline, the conference participants concluded: Strategic plans and priorities of the institutions and agencies should emerge from the exciting opportunities that exist across the spectrum of scientific fields. This means that the research community must be a part of the process from the beginning, with each community presenting plans and priorities for its discipline and not simply responding to plans presented at the institutional levels.
- Finally, the participants emphasized that the starting point of priority setting and strategic planning is understanding and stating clearly the objectives and goals of the particular institution or discipline. Universities, agencies, laboratories need clear mission statements.

The Roundtable, for its part, through a series of focus groups and national colloquia will attempt to convene the process I have described over the next two years.

It is unquestionably most difficult to carry out priority setting for the research enterprise as a whole -- to strike the right balance among investments in people, programs, and infrastructure for the Nation. How much is enough in terms of our research enterprise? Do we want scientific and engineering institutions which are comprehensive or specialized? And if we can reach a consensus on size and scope, achieving the desired balance would require the right people, enough money, sound decision making, a national communications infrastructure, and an open global research system. The Roundtable discussion paper "Fateful Choices," proposes a framework for considering these complex and often subtle issues. I hope you will have an opportunity to explore it in detail during your May 19th hearing.

In conclusion, let me offer some benchmarks for our stewardship of our precious and productive scientific research enterprise. We must always:

- Adhere to world class standards for scientific research;
- Make long term research commitments, and review and renew our progress on a regular basis:

- Nurture a sense of partnership between the Federal Government, universities, industry, and even state governments as co-venturers in the research enterprise;
- Assure support of high risk innovative research, and a diverse community of researchers;
- Employ stringent evaluation criteria to assure the quality of work being undertaken with public support.

Lord Kennet said recently: "Politics is the art of taking good decisions on insufficient evidence." I can think of no better standard to raise over these deliberations, as you wrestle with a process to achieve sound priority setting for our Nation's vital scientific research enterprise.

Thank you.

Mr. BOUCHER. Thank you, Governor Celeste.

Dr. Gomory.

Dr. GOMORY. I'm very pleased to have the opportunity to be here.

Mr. BOUCHER. Dr. Gomory, if you could move that microphone over in front of you, we will hear you a lot better. Thank you.

Dr. GOMORY. I will repeat my opening, startling message, which is that I'm very pleased to be here, and my basic message today is that setting priorities in scientific work can be done, but it cannot be done if we don't have goals, and if we don't know where we are going it is very hard to have a sensible discussion about the fastest way to get there.

Now my experience as a director of research, which I was for 19 years, has certainly convinced me that it is possible to set priorities between totally disparate areas such as memory and displays, et cetera, et cetera, but only if you know where you are going, only if you have goals. I'm inclined to believe that, in reality, it is a lack of agreed on goals that has complicated the setting of scientific priorities, so I will attempt to suggest some possible goals as I discuss the various aspects of Federal science support.

First, although I think the categorization of individual investigator versus mega-project is the wrong way to think about this subject, I will use it anyway, and then I will try and explain how we could look at it in another way. So I'll start by talking about support of the individual investigator.

It is clear, and I think completely unarguable, that support of the individual investigator has been enormously successful, the most successful by far of any money spent by the Federal Government in support of science. It has brought forth an understanding of solid state physics that led to the transistor, it has brought forth molecular biology with all its tremendous impact on biotechnology, and there is nothing comparable to this.

Nevertheless, in spite of this very generous support, very successful support, and enormously successful evolution with an impact on the entire world as well as on the subjects, there is a great deal of unrest and unhappiness in that scientific community that did that work, and if we try and understand what that is about we encounter a great deal of confusion. At one moment, we are told there is a shortage of scientists and engineers; the next moment, we are told there are too many of them, more than we intend to support in doing research at any rate.

So it is clear that we haven't got the basic facts about this picture. Are we generating too many people? Are we generating too few people? In fact, we don't know.

The second thing that is unclear is, what would we do if we did know? What would we be aiming at? What would our goals be for these individual investigators in physics, in chemistry, and so forth? Is it possible to articulate goals for basic science even if you have a clear picture of what is going on?

Now most of us who are scientists automatically, or at least semi-automatically, reject goals that set specific aims for scientific subjects, and we will say things like what you just heard, which is, you can't aim invention, but as a country we could set goals for our science and we could have a goal of being world class in most major scientific fields, by which I mean world class in physics, world class in chemistry, et cetera, and once that was said, I think we could articulate what it would take in the scientific community to do that and to have a decent life for the people involved at the same time.

So basic science has really worked, and it has undoubtedly benefited the world, and we should keep going at a good pace, but we should settle what the destination is that we are aiming toward.

Now let me go on to mega-projects. My remarks so far have been about the individual investigators, and I want to distinguish two types of mega-projects. The first type is scientific mega-projects, and the second type is mega-projects that are often referred to as science but, in fact, aren't.

Of the first type, the science mega-project, I would list, for example, the superconducting supercollider and various orbiting telescopes such as the Hubble, and scientific satellites and space probes. Now mega-projects in general have certain elements about them that command support, and I won't try and go through that with a group that probably understands it better than I do.

The mega-projects are expensive. For example, the various scientific satellites of NASA cost a few billion dollars a year, which is comparable, let us say, to the entire budget of the National Science Foundation, and these mega-projects are often good science, but the question is: Is this the right way to prioritize and spend science money on that scale?

Perhaps we could deal better with scientific mega-projects by incorporating their cost into the relevant field. In other words, the scientific satellites should not be viewed as something NASA does but something we do as part of astronomy, and to be world class in astronomy you do a certain amount with very expensive instruments and you do a certain amount with principal investigators. These are not to be regarded as mega-projects versus investigators; it should be regarded as, what do we do to be world class in astronomy, and I think the things should be weighed in that light, and you will find, of course, that in most fields it involves some major instrumentation and some investigators. That balance is different for each field, and I think the way to look at this is, what do you need to be first class in various parts of physics, of chemistry, biology, rather than mega-projects versus individual investigators.

Now let me go on to nonscience mega-projects. Space is the best example, although there is also the National Aerospace Plane. To understand the space program, at least from the peculiar perspective I will bring to it, you have to remember its history, all of which I remember. Who can forget the national reaction to Sputnick, for example? President Kennedy's decision to send men to the moon—and we did not send men to the moon to settle the startling question of what does the surface of the moon look like, we sent the men to the moon as part of a race in space with the Russians.

We should therefore ask today, what is the purpose, what is the goal, of our space program at a time when our rivalry with the Soviet Union, or the late Soviet Union, is clearly so diminished? and if we did ask that question, we would be told various scientific answers, among others—for example, that it is important science, that it recruits people into science, or that it contributes spin-offs to civilian technology. These explanations are a little bit true, but there is no sense in which they could possibly justify that scale of budget.

We could also be told—and here I think we are closer to the truth—that the manned exploration of space and perhaps the eventual settling of space by people is a national goal in itself quite separate from scientific considerations. But I would say if that is the goal that lurks behind the space program, let's bring it out and debate it as a national goal: Do we want to do this? and, secondly, if we decide to do it, which I would, in fact, be in favor of, let's do it at a pace which is appropriate for that national goal and not necessarily the pace which was formed and is continued as if it were a race with the Soviet Union. So, again, if we were to set a goal in this area, I think it would be much clearer what the appropriate rate of spending should be.

Now let me move then to the fourth area of Federal support of science and technology, which is science in support of national goals such as industrial competitiveness, weapons, environment, energy, and education. Of all these, I will speak only on competitiveness as it is the one on which I have some considerable experience.

In the U.S. in recent years, we have graduated from the idea that science alone guarantees industrial leadership to the idea that science and technology plus the rapid commercialization of new ideas are what matter. A striking example of this emphasis on new and high technologies, which many of you perhaps remember, was the tremendous stir a few years ago about high-temperature superconductivity. There was a tremendous agitation about this, there were meetings with the President involved, and so forth.

Behind all that is the thought that getting new technologies into product is the issue that we have ideas but others commercialize them. Unfortunately, that idea, attractive though it is, flies directly in the face of the fact; the fact is that the industries which make up the balance of payments deficit are textiles, automobiles, semiconductors, and consumer electronics, but the problems in all of these have had almost nothing to do with the commercialization of new technologies and everything to do with questions of manufacturing.

So that in the industries where we have had problems, they have been problems of quality, speed, and manufacturing, and these have been the real strength of our first-rate competition, much more so than the publicized advanced technology issues.

In this area of contributing to industrial competitiveness, we need to set a goal. A reasonable goal would be to contributing—of contributing to American industrial competitiveness through science and technology. We then need, in close cooperation with industry, to discover what science and technology would contribute to giving us competitive industry. We need to work back from the competitiveness goal rather than forward from the latest scientific event. The results will likely be a mix of the old and the new and of high-tech and of manufacturing, but it will be far more likely than what we do now to help competitiveness.

In sum, I believe that in all these areas—the investigator, the mega-projects, and this last, competitiveness—we will be able to set priorities if we first articulate our goals and we will not be able to set priorities that will last if we don't do that.

Thank you very much.

[The prepared statement of Dr. Gomory follows:]

Goals and Priorities for the U.S. Government's Role in Science & Technology
by R. E. Gomory - The Alfred P. Sloan Foundation

(Testimony Prepared for a Hearing of the House Subcommittee on Priority Setting in Science)

Introduction

My basic message today is that setting priorities in scientific work is *not* uniformly difficult.

It can be very difficult however if we do not have clear goals. If we don't know where we are going, it is hard to have a sensible discussion about the fastest way to get there.

On the other hand, experience as a director of research has convinced me that when there *are* goals, sensible priorities can in fact be reached. In industrial research we deal with priorities between science and technology, within technology between logic and memory and displays, and within displays with gas panel v.s. the cathode ray tube. On these questions there was always conflict and different views, but usually, after discussion, a reasonable conclusion could be reached. This in the presence of fairly well understood, but not precisely understood, goals.

I am inclined to believe that in reality a lack of *agreed on goals* has complicated the scientific priorities discussion so I will attempt to suggest some possible goals as I discuss the various aspects of Federal Government Science support.

I. Support of the Individual Investigator

I will talk first about support of basic science, especially the individual investigator.

By any reasonable standard this has been enormously successful, and by far the most successful of the government roles. This policy of basic science support was a fruit of the post World War II period, when the great achievement of scientists during the war, for example the atomic bomb and radar, gave both politicians and the public a feeling, and in my opinion a correct feeling, for the immense power that could be unleashed by scientific knowledge.

And this thought - science is power - which led to this policy of support, was in fact rewarded by scientific successes that have transformed and continue to transform the world.

I am thinking here of the transistor, an invention that grew out of the basic understanding of solid state physics, in the same way that the atomic bomb grew out of the understanding of the atomic nucleus, or equally of molecular biology with all its remarkable revelations and all its consequences as a technology.

When we seek to justify Federal money spent on the individual investigator we have an easy task. We don't have to look forward and speculate, we only need to look back at a great history of success. And it is success whether it is measured in terms of scientific progress or in terms of advancing the material level of the world.

Nevertheless, and despite that success, there are clearly problems today within the basic science community itself. There are high rejection rates at the science supporting agencies, such as NIH (the National Institutes of Health) and NSF (The National Science Foundation), a diminution of interest in science and engineering on the part of students, a long pipeline to the Ph.D and, some difficulty getting jobs at the other end of that long pipeline. So despite a remarkable record of success, we may not be producing a reasonable way of life for scientists.

In trying to understand what is going on and what to do about it we immediately encounter confusion.

Some say the answer to the high rejection rate is simple, scientists clearly do good, we should simply give them more money. We should fund any good idea because its worth it.

Others say that the money spent on science has been in fact increasing steadily and to increase it more under the present ground rules will produce an ever increasing population of research scientists who will be claimants for the same limited number of desirable jobs, and provide still more competition for grants.

The remarkable fact is that in fact we don't know what is going on. We don't have the most basic model of the process of generating researchers. We don't know how many there are out there. We simply don't know what is happening today. As a result, what does happen is much more a political process than a thought out process.

What we would do if we had a decent picture is also unclear. What would our goals be? Is it possible to articulate goals for basic science anyway even if you have a clear picture of what is going on?

Most of us automatically reject goals that set specific aims for scientific subjects. But, as a country we could set goals in a different way. *We could have a goal of being world class in most major scientific fields*, and at the same time having a decent life for those who pursue basic research. Then we could list these fields, see what it takes to be world class, and try to get it. We could estimate, debate, and work toward such goals. Today we don't have such a process, we don't have such a debate, and in addition we don't have reasonable data.

Basic science and the Federal government support of it has really worked. It has undoubtedly benefitted the world. We should keep going. But we should stop flying blind toward an unknown destination, for the good of the researchers themselves as well as for the rest of the world.

II. Support of Megaprojects

Two types of Megaprojects.

Next I would like to say a few words about megaprojects. I will talk about two types of megaprojects; those that I call real science, and those that are often referred to as science, sometimes justified as science, but aren't science.

II-A. Science Megaprojects

Of the real type I would list for example, the Superconducting Supercollider, various orbiting telescopes, and other scientific satellites and space probes.

Any megaproject has certain curious elements of natural support, which in a goal free world the individual investigator doesn't. It is intelligible (at least compared to more general basic research) and exciting, and it spends money in someone's home district or home state, and as such it is competitive with other forms of home district government spending.

Then there is the support through the ongoing actions of a government agency. NASA and DOE are examples.

These are large, i.e. multi-billion dollar organizations, that are driven by natural desires for continuing their work to propose and powerfully advocate a succession of megaprojects. For example, the various *scientific* satellites of NASA cost a few billion a year.

It would only be fair to observe that the support of individual investigator basic research is aided by the same institutional factors or institutional autonomy of the agencies, such as NSF and NIH, that support that kind of work. It is a characteristic of our present system that the moneys spent in these different ways are not compared.

Often this kind of megaproject is good science. But the question is, is this the right way to prioritize and spend our science money. After all two Billion a year on space probes compares with the total amount that NSF spends on individual investigators. And historically the individual investigator has been far more productive.

Perhaps we could deal better with *scientific* megaprojects by incorporating their cost into the relevant scientific fields, astronomy, or earth sciences, or physics, and making sure that this is the way we want to spend money to obtain world class standing in that field. I believe that with that goal in mind a sensible debate could ensue.

<u>II-B. Non-Science Megaprojects</u>

Then there is the non-science megaproject. Space is the best example. Although there is also the National Aerospace Plane.

The space program originated in our race with the Soviets. Who can forget the extreme national reaction that greeted Sputnik. Edward Teller, in his usual picturesque way, asserted that we had suffered a defeat worse than Pearl Harbor.

Out of this disturbed national atmosphere came a political decision to put men on the moon. And we did put people on the moon,, and we did it to surpass the Soviets, *not* to settle the question of what the surface of the moon looks like.

We could wonder, given this capsule view of the origins of the space program, whether such a program is necessary today, when the rivalry with the Soviet Union is so diminished. After all we are spending more money on the space program than the combined budgets of three NSFs and one NIH all added up.

If we did ask that question we would get more than one answer. We would be told, for example that the Space Program is:

a) Important science
b) That it recruits people into science
c) That it contributes to civilian technology

These explanations are all science and technology oriented, they are all somewhat true (or slightly true), but it is clear, at least to me, that they come nowhere near justifying a 14 billion dollar a year price tag on the basis of science and technology goals.

We could also be told, and here I think we are closer to the truth, that the manned exploration of space, and perhaps the eventual settling of space by people, is a national goal in itself quite independent of science. But if it is a national goal, to explore or settle space in this way, then let us articulate this goal, and debate it, rather than obscuring it with scientific justification. And, if we accept this national goal, let us also decide to pursue it at a proper pace, which would not necessarily be the pace appropriate to a race with the Soviets.

In contrast to basic science, space, whatever its rationale - doesn't work, or more accurately it doesn't work or perform some obvious useful function now, in the absence of an intense Soviet-American rivalry. For this reason we need to clarify what we are doing. There is no science that could justify the enormous bill, and if the goal is something else, like manned exploration of space, let's talk about that and about its pace and rate of expenditure.

III. Science in Support of National Goals such as Industrial Competitiveness - Weapons- Environment - Energy - Education

I will confine myself to talking about government efforts to support industrial competitiveness.

Competitiveness

In the U.S. in recent years we have graduated from the idea that science alone guarantees industrial leadership to the idea that science and technology plus the rapid commercialization of new ideas are what matter.

Innovation is now an important word. Time magazine had a special issue on industrial competitiveness. It was entitled "Innovation in America", almost as if innovation and industrial competitiveness were synonyms.

The Federal government is moving from a position of supporting only basic science support to a position of supporting "generic" or precompetitive technologies. Lists of key technologies abound, coming from both government and private sources (like the Council on Competitiveness). The implication of all these lists is that these are the technologies that are the keys to competitiveness.

A striking example of this emphasis on advanced technology occurred a few years ago when high-temperature superconductivity appeared on the scientific scene. There was a major government reaction. There were public meetings with the President attending to discuss the subject of superconductivity. There was very strong sentiment that, in this area, we couldn't let the Japanese do it to us again.

Though somewhat less extreme, there was a similar reaction to the Japanese 5th generation computer plan, which in fact produced a worldwide, as well as an American, reaction

Behind all this is the thought that getting new technologies into product is the issue, we have ideas, but others commercialize them. <u>If new technology commercialization were really the problem</u> it would be very convenient, because it would allow us to use a science and technology policy as a substitute for an industrial policy, and industrial policy in a broader sense is and has been a complicated and questionable subject in the U.S. The reason for this questionableness being less current policies or current politics, but more importantly, the actual fundamental abilities and *inabilities* of the American Government.

Unfortunately this view of the problem flies directly in the face of the facts. The U.S. has not had an innovation problem to date, even in a commercialization sense. The industries which make up the balance of payments deficit are textiles, automobiles, semiconductors, and consumer electronics. I will not comment on textiles as I know nothing about them, but the problems in the other three have had little to do with innovation and everything to do with manufacturing.

These are not industries where we have had ideas and others commercialized them, they are all industries which did commercialize the original ideas and had a strong position in the grown up industry itself, but later lost out that position to competitive products with superior quality, lower manufacturing cost, and to competition having a rapid development cycle leading to rapid incremental improvement in the product.

To date quality, speed, and manufacturing have been the real strength of the competition, rather than the much more publicized MITI advanced technology efforts, and until we face that reality we are unlikely to make progress.

In this area, as in the others, we need to set a goal - the goal of contributing to American industrial competitiveness through science and technology. We then need, in close cooperation with industry, to discover what science and technology programs will contribute to giving us competitive industry. We need to work back from the competitiveness goal rather than forward from the latest scientific event. There will be different views and discussions, but I believe a sensible outcome would emerge. The result will likely be a mix of the old and the new, of high tech and of manufacturing technology. But it will be more likely than what we do now to help competitiveness.

In sum, I believe that in all the areas we can set goals, and then we will be able to set priorities, and if we do not set goals we will *not* be able to set priorities in a sensible and lasting way.

Thank you very much.

Mr. BOUCHER. Thank you, Dr. Gomory.

Dr. Brooks.

Dr. BROOKS. Thank you very much.

I would like to—

Mr. BOUCHER. Dr. Brooks, if you could move the microphone a bit closer, that would help us.

Dr. BROOKS. Yes. I would like to comment briefly on a few of the issues that apparently emerged at the recent hearings which cause me a certain amount of difficulty in trying to understand the conclusions.

First, it was somewhat unclear to me what was the universe within which we were trying to set priorities for R&D. Is it the entire Federal R&D budget of about $70 billion? just the part that is labeled "research," about $12 billion? or just academic research that includes the research carried out in national labs such as Fermilab or the National Astronomy Observatories that can be considered for all practical purposes as simply extensions of academia?—in other words, multi-purpose research sponsored and performed with goals defined primarily in scientific rather than social purpose terms.

Secondly, there seems to be an implicit assumption running through the previous hearings that the choices being made are within or between scientific disciplines or fields of research. That suggests to me that we are talking primarily about the Federally supported part of academic research, about $10 billion, rather than the total Federal R&D budget or even the total Federal research budget.

Certainly choosing among disciplines doesn't make much sense if you are talking about development or even most of mission-oriented research—that is to say, research with a goal defined primarily in terms of a single social objective. In this case, the disciplines are complementary within a mission and are really not in direct competition with each other; they simply are derived from a judgment about what is necessary to accomplish the mission. The mix of disciplines derives, in fact, from the particular missions or submissions that we are talking about. On the other hand, mission oriented R&D uses the disciplines and, in using them, does contribute somewhat to their development, and it is unclear just how much that should be taken into account.

To take one example, NIH supports 45 percent of all academic research and supports considerably more chemistry than NSF, but generally it has different priorities among chemistry sub-disciplines, much more of a focus on biochemistry, for example, than NSF has. Similarly, NASA uses a great deal of physics but probably contributes much less to the advance of physics as a discipline than NIH contributes to the advance of chemistry as a discipline. So there is a serious question of how much of the physics and chemistry that NASA and NIH support in their own intramural labs, for example, as part of their missions, should count as Federal support for physics and chemistry in the sense of an overall Federal disciplinary portfolio.

This relates to the point that was raised by Kumar Patel in one of the hearings as to who is the customer for the research. For example, is it primarily scientists in other disciplines, or is it primarily engineers or clinicians or other professionals who use scientific knowledge in delivery of professional services to clients? Some disciplinary research serves only a single customer or a very restricted range of customers, whereas other disciplinary research serves such a broad variety of customers that it is almost meaningless to talk about a customer at all. Rather, it has a multiplicity of potential customers not predictable in advance.

In a program such as the Global Change Program which was cited in the hearings as a good example of priority setting, successful priority setting, the distribution of research support among the disciplines is simply a secondary by-product derived from a societal goal of understanding the causes and future evolution of global environmental change with a view to developing response policies. In such a case, the distribution of support among disciplines can be, in fact, determined rather—quite scientifically.

Perhaps part of the problem of setting priorities derives from the difficulty of deciding what a given scientific project or research area should be compared with. Should it be compared with other, largely unrelated science—high-energy physics versus molecular biology, for example—or should it be compared with other, less technical means of achieving a given societal goal? Should the comparison between two scientific projects be made on the basis of a judgment as to the relative value of the societal goal which each mainly contributes to, or should it be made on the basis of the intrinsic scientific merits of each of the projects, however defined?

In the first case, the role of scientist is mainly to assess the degree in which each program is likely to contribute to its claimed societal goal, but only laymen and politicians can decide on the relative importance of the respective societal goals to which each program is alleged to contribute. Of course, scientists have a part in this because they are also citizens. On the other hand, it would be absurd to ask scientists to decide whether military research is more important than biomedical research or vice versa. Although they may have views, this is a purely political decision.

From these remarks, it seems to follow that the notion of an overall R&D portfolio within which trade-offs are made does not make much sense. The more the goals of scientific programs are defined in societal rather than technical terms, the more necessary it is to consider trade-offs outside as well as within R&D. I do not think it reasonable, for example, to talk about how much R&D the country can afford, although it would be much more reasonable to talk about how much academic research it could afford or how much academic research was reasonable in the light of the overall volume of national R&D spending, public and private, as determined by the societal missions of the agencies and corporate entities that spend it.

Only if overall R&D spending as derived from the aggregate of all societal missions begins to exceed the capacity of the R&D infrastructure to support it might it be reasonable to talk about trade-offs within R&D. That problem has arisen only once in recent history so far as I know—namely, in the early phases of the Kennedy administration when the simultaneous build-up of the Apollo program and the new ballistic missile programs did show strong signs of overtaxing the technical infrastructure.

I am not, therefore, particularly excited about the discussion of which agencies should be involved in setting scientific priorities. I think the questions that I have outlined above regarding which programs it is reasonable to compare, what the envelope should be within which various kinds of trade-offs are made, and how technical activities should be categorized or classified from the standpoint of prioritizing have to be decided before one can talk meaningfully about what agencies should do it and who should be involved.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Brooks follows:]

Testimony for April 28

Harvey Brooks

I would like to comment briefly on a few of the issues that apparently emerged in the recent hearings, which caused me a certain amount of difficulty in trying to understand the conclusions.

1) First, it was somewhat unclear to me what was the universe within which we are trying to set priorities for R&D. Is it the entire federal R&D budget of about $70B, just the part that is labeled research (about $11B), or just academic research that includes the research carried out in national labs. such as Fermilab or the National Astronomy observatories that can be considered for all practical purposes as extensions of academia? In other words, multipurpose research sponsored and performed with goals defined primarily in scientific rather than social purpose terms and focused mainly in academia.

2) Second, there seems to be an implicit assumption that the choices being made are within or between scientific disciplines or "fields of research," which suggests to me we are talking primarily about the federally supported part of academmic research (about $10B). Certainly, choosing among disciplines doesn't make much sense if you are talking about development or even most of "mission-oriented" research, i.e. research with a goal defined primarily in terms of a single societal objective. Here the disciplines are complementary within a mission and are really not in direct competition with each other; the mix of disciplines derives from the particular missions or sub-missions. On the other hand, mission-oriented R&D <u>uses</u> the disciplines, and in using them contributes to their development, so the disciplinary choice is not completely irrelevant either. To take one example, NIH supports 45% of all academic research, and supports more chemistry than NSF, but generally has different priorities among chemistry subdisciplines than NSF has (e.g. a focus on biochemistry and molecular biology). Similarly, NASA uses a great deal of physics, but it probably contributes much less to the advance of physics as a discipline than NIH contributes to chemistry as a discipline. So there is a question of how much of the physics and chemistry that NASA and NIH support, especially within their own intramural labs as part of their missions, should be "counted" as federal support for physics and chemistry as part of an overall federal disciplinary "portfolio." This relates to the point raised by Patel as to who the "customer" is for the research. For example, is it primarily scientists in other disciplines, or is it primarily engineers or clinicians, or other professionals who use scientific knowledge in delivering professional services to clients? Some disciplinary research serves only a single customer or a very restricted range of customers, whereas oher disciplinary research serves such a broad variety of customers that it is almost meaningless to talk about a customer at all; rather it has a multiplicity of potential customers not predictable in advance.

In a program such as the global change program, used as a good example of priority setting in the first hearings, the distribution of research support among disciplines is a secondary by-product derived from the societal goal of uncerstanding the causes and future evolution of global environmental change with a view to developing response policies. In such a case the distribution of support among disciplines can be determined fairly scientifically through a scientific analysis of the societal goal.

3) Part of the problem of setting priorities derives from the difficulty of deciding what a given scientific project or research area should be compared with, whether within science or with activities outside of science itself. Should it be compared with other, largely unrelated science, or should it be compared with other less technical means of achieving a given societal goal? Should the comparison between two scientific projects be made on the basis of a judgment as to relative value of the societal goals which each mainly contributes to, or should it be made on the basis of the intrinsic scientific merits of each of the projects, however defined? In the first case, the role of scientists is mainly to assess the degree to which each program is likely to contribute to its claimed societal goal, but only laymen and politicians can decide on the relative importance of the respective societal goals to which each program is alleged to contribute. On the other hand, it would be absurd to ask scientists to decide whether military research is more important than biomedical research or <u>vice versa</u>, unless the argument had to do with a specified outcome such as commercial spinoff. Otherwis it is a purely political decision.

From the above remarks it seems to follow that the notion of an overall R&D portfolio within which all trade-offs are made does not make much sense. The more the goals of scientific programs are specified in societal rather than technical terms, the more necessary it is to consider trade-offs outside as well as within R&D. I do not think it is reasonable, for example, to talk about how much R&D the country can afford, although it would be more reasonable to talk about how much academic research it could afford, or how much academic research was reasonable in the light of the overall magnitude and pattern of national R&D spending, public and private, as determined by the societal missions of the agencies and corporate entities that spend it. Only if overall R&D spending as derived from the aggregate of all societal missions begins to exceed the capacity of the R&D infrastructure might it be reasonable to talk about trade-offs <u>within</u> the R&D "envelope." That problem has arisen only once in recent history, to be the best of my knowledge, namely in the early years of the Kennedy administration when the simultaneous build-up of the Apollo program and the new ballistic missile programs of the military did show signs of overstretching the technical infrastructure.

4) In the light of the preceding considerations, I cannot become very excited about the discussion of which agencies should be involved in setting scientific priorities. I think the questions outlined above regarding

what programs it is reasonable to compare, or what the "envelope should be within which various kinds of trade-offs are made, and how technical activity should be categorized or classified from the standpoint of prioritizing have to be decided before one can talk meaningfully about what agencies should do it, and who should be involved.

Mr. BOUCHER. Thank you, Dr. Brooks.

Dr. Dutton.

Dr. DUTTON. Thank you, Mr. Chairman, and thank you for inviting me to testify at this hearing.

As you know, the Space Studies Board Task Group I chair has just released a report, "Setting Priorities for Space Research: Opportunities and Imperatives," that addresses the issue of whether the space research community should help to set long-range priorities for space science and applications. Our conclusion was a resounding yes. Not only is it desirable, it is imperative.

In our deliberations, we were inspired by a quotation by Metternich brought to us by a member of the Task Group, Buddy MacKay, a former colleague of yours and now lieutenant governor of Florida:

"Policy is like a play in many acts which unfolds inevitably once the curtain is raised. To declare that the performance will not take place is an absurdity. The play will go on, either by means of the actors...or by means of the spectators who mount the stage."

In my remarks today, I will set the context for our report, summarize a few of our conclusions, and outline the next and more difficult phase of our study.

Let me first state my personal view of how our report fits within the context of the national decision-making process. Priorities for space research or for a national science program appear within a hierarchy that ranges from national goals to individual research projects. At the top of the hierarchy are national goals and objectives, such as developing deeper understanding of the world around us or enhancing economic vitality. Next come the strategic endeavors or initiatives that contribute to the achievement of national goals. Examples might include the study of global change induced by human activities or the development of enhanced computer and information technology. At the third level are specific research programs, space research missions, or technology development programs.

To consider priorities, we must divide the specific initiatives into two categories, the longer-range, conceptual or potential efforts, and the more immediate or programmatic activities.

In space research, programmatic activities include building spacecraft, flying them, and using the data they return in research. Conceptual efforts concentrate on developing new ideas and new approaches for attacking scientific questions. They explore mission concepts, refining them into proposals for programmatic activities.

Some years ago in a NASA Science Advisory Committee, we developed a methodology for setting programmatic priorities within NASA's Space Research Program. It has been quite effective. In our present report, we argue that we must now address the more difficult task of recommending priorities for a long-range program, developing a procedure for combining proposals from various disciplines into a comprehensive agenda a decade or so in advance.

In my opinion, in order for scientists and public officials to shape an effective national science program, national goals and purposes must be clear. If we are vague about national goals and strategic priorities or if these goals shift about, changing emphasis, then we shall waste money and effort as we start projects and later cancel them. I'm tempted to quote from Yogi Berra, "If you don't know where you're going, you may end up somewhere else."

Relative to the purposes of the National Space Program, our committee believes that the imperative driving scientific research is the acquisition of knowledge and understanding. Thus, the collection of data, the creation of information through its analysis, and the subsequent development of insight and understanding should be the key governing objectives for scientific research in space and for the broader space program.

We believe that the Nation would benefit if space research and much of the space program emphasized the acquisition of information and knowledge, the development of insight, and understanding. Adopting the acquisition of information that cannot be obtained on earth as the primary purpose of space activities is compatible with national needs to develop advanced technologies and capabilities. Most significantly, such a purpose provides clear objectives for future development of the human space flight program.

Let me go on now to the justifications for long-range priorities that we set forth in our report. First, consensus is politically compelling. When scientists demonstrate that their agenda responds to both scientific imperatives and to national needs, then they can argue effectively for an adequate share of resources and for an orderly progression through an agenda endorsed by the community.

Second, as Metternich said, if the players will not act, then the spectators will take the stage. If scientists cannot or will not set priorities among opportunities, then others whose own goals may be quite different will take the stage and make the decisions.

In order to prepare an effective long-range agenda, we will need a sophisticated system of priorities; a simple ranked list will not be sufficient. Thus, we envision a hierarchical scheme with certain classes of activities given a higher priority than others. We describe such a scheme in our report. In creating it, we must remember that a collection of small efforts may, in sum, be of greater value than a single large effort.

Our report thus urges scientists to accept the responsibility of participating in the decision-making debate. As encouraged by Congressman Brown in a recent address at the National Academy of Sciences, we must provide policy makers with our best assessment on priority ordering based on unadulterated peer review judgment of scientific merit.

In the course of our study, we encountered arguments against scientists participating in the setting of priorities. I'll mention some of these objections and then our counter-arguments to them. The first objection we hear is, there will be losers. Yes, there will be, just as there are losers now. But consensus in the scientific community along with effective advocacy will, in all likelihood, produce more funds and more stable funding patterns.

The second objection is that recommending priorities is too difficult, too contentious. Sure, it is difficult, but we believe it can be accomplished through a formal process utilizing explicit criteria addressed with written proposals.

A third objection is, the low-priority initiatives will not be done. Exactly; that is the purpose of setting priorities; we want to favor the highest priority endeavors. And, lastly, scientists cannot make political judgments. We believe that scientists should be sensitive to national goals and to political realities just as we expect the politicians in considering scientific initiatives should be sensitive to scientific merit.

During the next year, we will be developing a procedure for recommending priorities for a vigorous, long-range space research agenda. We are well aware that the most difficult part lies ahead. Many questions must be answered. For example: What criteria are appropriate for determining priorities in a long-range developmental agenda for space research? How should the process be structured, and to whom should the recommendations be addressed? How can we determine what budget limits, maximal or minimal, should be placed on the totality of efforts considered in a developmental agenda? To what extent should we narrow the choices as we approach setting the programmatic agenda? These are just a few of the questions we must answer.

Evidently, we have set ourselves a difficult task. However, we believe it would be a serious mistake not to try. The community is capable of making the sophisticated judgments necessary to foster a vital and robust space research program. We believe it must do so.

Thank you very much for your interest and attention.

[The prepared statement of Dr. Dutton follows:]

SETTING PRIORITIES IN SPACE RESEARCH

Statement of

JOHN A. DUTTON
Dean, College of Earth and Mineral Sciences
Pennsylvania State University
Chairman, Task Group on Priorities in Space Research
Space Studies Board
National Research Council

Mr. Chairman, members of the Subcommittee. Thank you for inviting me to testify at these important hearings on behalf of the Task Group on Setting Priorities in Space Research, a committee of the Space Studies Board, National Research Council.

As you know, we have just released a report, *Setting Priorities for Space Research -- Opportunities and Imperatives*[1]. That report is the culmination of a two-year study which focused on whether the space research community should have a role in setting priorities for those scientific objectives and initiatives which comprise the space science and applications component of the nation's civil space program. Our conclusion was a resounding Yes. Not only is it desirable -- it is imperative. That it took nearly two years to convince ourselves, the Board, and other colleagues from the space community of the validity of this conclusion indicates the sensitivity and difficulty of this issue.

In our deliberations, we were inspired by a quotation from Metternich brought to us by a task group member, Buddy McKay -- one of your former colleagues, now Lt. Governor of Florida.

> [Policy] is like a play in many acts, which unfolds inevitably once the curtain is raised. To declare that the performance will not take place is an absurdity. The play will go on, either by means of the actors...or by means of the spectators who mount the stage.

In my remarks today, I will set the context for our report, give a brief overview of its conclusions, and outline how we plan to approach the second phase of this study -- by far the more difficult enterprise.

THE KEY QUESTIONS IN SETTING AN AGENDA

Each of you is well aware that, in sum, the requirements and opportunities competing for federal support far exceed available funding. We know that too. We also know that scientific research is an investment in this nation's future, not an entitlement program.

In our report, we document a wide array of remarkable achievements of the U.S. space research program over the past thirty years. We go on to describe some of the abundant opportunities that exist now and for the future. NASA charts depicting funding levels required just to complete the ongoing program, let alone begin new projects, are a graphic reminder of the very real need to make difficult choices. The community of scientists engaged in research in space must reach a consensus on priorities and contribute to the formulation of an agenda for space research

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[1] Space Studies Board, National Academy Press, 1992, Available from the Board.

and for the space program. Such an agenda and the priorities it represents must respond to national needs and to the larger priorities imposed by national goals.

The two key questions in space research, as in most continuing endeavors, are: *What should we do? How should we do it?* We set our agenda with the answers to these questions -- the priorities that we choose reflect our goals and our values. Careful consideration and formulation of assumptions and priorities for the scientific research program and the overall space program that supports it will enable us to better serve national goals, compel effective action, achieve the maximum return on our national investment, and foster public pride and confidence.

THE HIERARCHY OF PRIORITIES

Let me state my personal view of how the issues addressed by our report fit within the context of the national decision-making process that creates the agenda for scientific activities. These ideas will be discussed as we proceed with the second phase of our study. Priorities for space research or for a national science program appear within a hierarchy that ranges from national goals to specific research projects.

- **National Goals**. At the top of the hierarchy are national goals and objectives, such as developing deeper understanding of the world around us, strengthening education of young citizens, enhancing economic vitality, and preserving the environment. Priorities for such goals obviously evolve, but the time scale on which they are pursued will usually be decades or longer and may extend to centuries.

- **Strategic Endeavors**. Next are the strategic endeavors or initiatives that encompass or facilitate a collection of activities intended to contribute to the achievement of national goals. Examples might include the fight against disease, the study of global change induced by human activities, the development of enhanced computer and information technology, the scientific exploration of the solar system, or the conservation of energy. Strategic endeavors are pursued over time scales of years or decades.

- **Specific Initiatives and Activities**. At the third level are the initiatives and continuing activities through which we actually achieve the aims of strategic endeavors. These include specific research programs, space research missions, technology development programs, or development of new research facilities. The conceptualization, development, and implementation of these initiatives may take years, or perhaps, more than a decade.

In order to consider priorities effectively, we must divide these specific initiatives into two categories: conceptual or potential efforts and programmatic activities. We formulate the agenda for future programmatic activities by selecting those potential efforts to pursue -- we thus decide what we shall do. In setting a programmatic agenda, we determine how we shall do it.

In space research, programmatic activities include on-going research and the design, construction, and flight of spacecraft and the use of data from such flights. Examples of programmatic activities include implementing mature mission proposals such as those for the Advanced X-Ray Astronomy Facility (AXAF) or the Earth Observing System (EOS). Conceptual efforts concentrate on developing new ideas and new approaches for attacking scientific questions; they examine the possibilities for utilizing technological advances to obtain scientific information from space. In brief, they explore mission concepts, refining them until they evolve into proposals for programmatic activities. Developmental or conceptual efforts might be typified by studies of an astronomical facility on the moon, a suite of robotic missions to install scientific instruments on Mars, a mission to Pluto, or a constellation of geosynchronous satellites for continuing surveillance of the Earth and its atmosphere.

Within space research, priorities for programmatic activities have been developed in recent years by the Space Science and Applications Committee using a methodology created by its predecessor, the Space and Earth Science Advisory Committee[2]. So far, there has been no formal effort to set priorities among developmental efforts across all of space research. The disciplinary committees of the Space Studies Board have regularly set forth long-range research strategies with scientific goals and objectives for each of the subdisciplines of space research. These have not, however, been refined into an overall development plan with clear priorities. It is the difficult task of recommending priorities for such a long-range development program that we address in our report, *Setting Priorities in Space Research.* We need to develop a procedure for creating our agenda a decade or so in advance so that we know with confidence precisely what we intend to do, so that we can concentrate on the highest priority endeavors.

I would argue that the extent to which the scientific community and public officials can shape an effective national program in space research depends in part on how clearly we understand and can enunciate the higher-level goals or objectives which we hope to serve. If we are vague about our national goals and strategic priorities, then it is difficult to focus development and programmatic activities to achieve them. If our national goals and strategic priorities shift about from one emphasis to another, then we shall waste money and effort in program development and execution as we start projects and then later cancel them. In our report we discuss the importance of fundamental assumptions in shaping priorities -- these assumptions elucidate the basic motivations for what we are trying to accomplish and they must derive from, and serve the higher purposes of,

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[2] For a description of this methodology see: *The Crisis in Space and Earth Sciences -- A Time for a New Commitment* (NASA Advisory Council, 1986) and Dutton, John A., and Lawson Crowe, 1988. "Setting Priorities Among Scientific Initiatives." American Scientist 76: 599 - 603

space research or science. The more clearly those purposes are formulated, the more effective our system of priorities for scientific endeavors will be.

The remainder of my remarks are based on discussions and conclusions of the Priorities Task Group.

INFORMATION, KNOWLEDGE AND UNDERSTANDING

We examined the role of fundamental assumptions in shaping the civil space program. For some time, the objectives of the space research community and those of the broader space program have been in conflict. Apollo demonstrated national technological superiority at a critical time. A fundamental assumption of the civil space program developed in that era asserts that it is human destiny to explore the Solar System and perhaps beyond. New realities of international competition, domestic politics, and economics suggest the need to examine our assumptions to ensure that space research and the space program contribute effectively to national vitality.

We believe that the imperative driving scientific research is the acquisition of knowledge and understanding. Thus the collection of data, the creation of information through its analysis, and the subsequent development of insight and understanding should be the key governing objectives for scientific research in space and for the broader space program. We believe that the nation would benefit if space research and much of the space program emphasized the **acquisition of information and knowledge and the development of insight and understanding**. Adopting the acquisition of information that cannot be obtained on Earth as the primary purpose of space activities is compatible with national needs to develop advanced technologies and capabilities. Most significantly, such a purpose provides clear objectives for future development of the human spaceflight program.

ECONOMIC REALITIES AND THE MANAGEMENT OF AVAILABLE RESOURCES

Today, as federal dollars become increasingly scarce, demands for clear benefits from public investments and for effective use of available resources confront the space science and applications community.

Two trends in public policy offer both challenge and opportunity to space science. First, there appears to be an increased willingness to support activities primarily producing broad social benefits, as evidenced by policy and action motivated by concerns for clean water and air, for protecting the environment, and for maintaining wilderness, wildlife and habitats. Second, there is an increasing demand for publicly supported activities to provide explicit evidence that the benefits to be achieved merit the costs. Responding to these demands requires careful thought to demonstrate how space research or other scientific effort that fundamentally serves to augment knowledge and understanding contributes to society; it requires careful analysis to answer questions such as, *In what way and by how much does space research further national objectives?*

Economic benefits have been cited as a rationale for space research since the inception of the U.S. civil space program, yet the precise meaning of "economic benefit" has not always been clear. The narrowest definition would include strictly commercial activity that is profitable in the business sense. The case most often cited is that of commercial communications satellites, in which economic benefits can be defined as the value consumers place on the service and are measured by industry revenues.

We do not offer a formal cost-benefit analysis for scientific research in space. That was both beyond our charge and is difficult to do. However, from the perspective of setting priorities for space research initiatives, many requirements of cost-benefit analysis are instructive. Both those who propose research initiatives and those who review them should, as far as possible, identify all costs and benefits, determine the necessary conditions for success, estimate the probabilities and the consequences of failure, and specify the expected outcomes. While we are aware that many people object to any attempt to quantify science and knowledge, we believe this sort of analysis must be factored into any effective priority setting procedure.

In parallel with demonstrating the benefits of space research, we must be sure that we use the available resources wisely and efficiently. Many observers have emphasized that space research efforts seem to cost too much, take too long, and all too often fail to meet their original objectives. In recent years, we have forced scientific missions into launch modes that dramatically increased their costs and reduced their effectiveness. We diffuse our support for science by attributing scientific motivations to efforts that, while they serve legitimate public purposes, are essentially nonscientific. In our report, we discuss some of the lessons we have learned in three decades of space research and some of their implications for the future.

RATIONALE FOR SETTING PRIORITIES

We argue that there are two principal justifications for working toward a consensus and recommending priorities: First, *Consensus is politically compelling.* If scientists can demonstrate that their agenda responds to national needs and to scientific imperatives, then they can argue effectively for an adequate share of resources and for an orderly progression through the suite of initiatives endorsed by the community. Second -- as Metternich said, *If the players will not act, then the spectators will take the stage.* If scientists engaged in space research cannot, or will not, set priorities among opportunities, then others whose own goals may be quite different will take the stage and make the decisions. Passivity or disarray on the part of the scientists presents the political process with the opportunity, indeed the necessity, to make choices, some of which may not be in the best interests of science.

In order to prepare an effective developmental agenda, we will need a sophisticated system of priorities. A simple ranked list will not be sufficient. We envision a hierarchical scheme, with certain categories of activities given a higher priority than others. The categories in such a scheme might include support for basic research and scientific infrastructure, followed by mandatory efforts, large initiatives, and incremental efforts that are part of the forward march of science. The relative priorities in such a scheme can be presented as a matrix, with the columns representing categories and containing activities listed by relative priority within the category.

There are not now, nor are there ever likely to be, sufficient resources to do everything we would like to do. It is time for the proponents and the recipients of federal research support to step up to the challenge of participating in the decision-making debate. As scientists and engineers, we have the unique capability of examining our own scientific and technological goals and objectives from a vantage point as experts in the field. We must, as encouraged by Congressman Brown in a recent address at the National Academy of Sciences, provide policy makers with our best assessment of priority ordering based on "unadulterated peer-reviewed judgment of scientific merit".

COUNTER-ARGUMENTS TO THE COUNTER-ARGUMENTS

In the course of our study and since the publication of our report, we have encountered a remarkably uniform set of arguments against scientists participating in setting priorities. Not surprisingly, some find the notion of setting priorities threatening. Anticipating counter- arguments, we offered a response to those arguments in our report. Below, I list some of the objections, and then our counter-arguments to them.

There will be losers. Yes, there will be, just as there are losers now. Consensus in the scientific community along with effective advocacy will, in all likelihood, produce more funds and stable funding patterns and hence strengthen science and increase opportunities for the recommended initiatives. Without a process that identifies and promotes good science and strong initiatives, resources are scattered and the strong subsidize the weak.

Recommending priorities is too difficult, too contentious. Yes, it is difficult. But we believe it can be accomplished through a formal process in which competing initiatives are judged uniformly according to explicit criteria, preferably on the basis of written material that specifically addresses the stated criteria. Again, if scientists find it too difficult to create an agenda for space research, then, as argued above, others will do it for them.

The community will not be able to maintain consensus because those who lose will subvert the process by lobbying policy makers and Congress directly. We argue that rather than seeking to restore initiatives that have been abandoned, those who lose out in the process would be better advised to develop more competitive initiatives.

Setting priorities will be counterproductive because the community will tear itself apart. We believe that insisting on a fair, open and formal process will, in the end, serve both individual scientists and science at large. If the space research community is to be taken seriously by others, then it should accept responsibility for its own future.

The low-priority initiatives will not be done. Exactly -- that is the purpose of setting priorities. When resources are limited, they should be directed toward the highest-priority endeavors.

Scientists cannot make political judgments. We believe that in arguing for initiatives, scientists should be sensitive to national goals and political realities, just as we expect that politicians in considering scientific initiatives should be sensitive to scientific merit. Since scientists expect support from taxpayers, they should be willing to explain to the public why some initiatives better serve national purposes.

THE DIFFICULT PART

Having begun the second phase of our study, we are well aware that the most difficult aspect of our endeavor lies ahead. Over the next year, we will be developing a procedure for recommending priorities that will contribute to the creation of a vigorous long-range space research agenda. We understand that for such a procedure to be successful, it must be accepted by the space research community at large while at the same time serving as a meaningful source of practical, reasoned advice to decision makers. It is our intention to actively involve the space research community in the development and testing of the methodology and implementation plan we create. That dialogue began earlier this year at a symposium marking the release of our phase one report.

Many issues and questions must be addressed and answered. For example:

(1) What are the appropriate criteria for determining priorities in developing a long-range agenda for space research or for other scientific endeavors?

(2) Who should be responsible for administering the process that is finally recommended?

(3) What will be the time schedule for the evaluation process and subsequent priority recommendations?

(4) To whom should evaluators' recommendations be directed Congress -- NASA...the Space Council...?

(5) How will the process provide for making choices within disciplines as well as across space research disciplines?

(6) Is it realistic to suggest that science can be subjected to any sort of cost-benefit analysis?

(7) How can we determine what budget limits (minimum and maximum), if any, should be placed in the totality of efforts considered in a developmental agenda? To what extent should we narrow the choices as we approach setting the programmatic agenda?

These are just a few of the questions we must answer. There will be more questions and more criticisms. Clearly, we have set ourselves a difficult task. However, we believe it would be a serious mistake not to try. Helping to fashion the appropriate criteria for making these difficult choices is, we believe, a responsibility of the space research community. The community is capable of making the sophisticated judgments necessary to foster a vital and robust space research program. We believe it must do so.

Copies of this report are available from

Space Studies Board
National Research Council
2101 Constitution Avenue, N.W.
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Setting Priorities for Space Research

Opportunities and Imperatives

Task Group on Priorities in Space Research—Phase One

Space Studies Board
Commission on Physical Sciences, Mathematics, and Applications
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Mr. BOUCHER. Thank you, Dr. Dutton, and the subcommittee expresses its appreciation to all of our witnesses for their very informative testimony here this morning.

Dr. Brooks, let me kind of set the stage for this round of questions by perhaps clarifying the mission that we have here. You had asked for some clarification of that, and I think that is a proper question. What we are focusing on is not the entire Federal research budget of some $70 billion that includes both defense and nondefense components.

What we are really looking at is the nondefense aspect of that budget, an amount that is roughly $30 billion per year, and that includes the civilian agencies such as the NIH, the National Science Foundation, the Department of Agriculture, the National Labs, the Environmental Protection Agency, Department of Energy, NIST, NOAA, NASA, et cetera.

We are examining whether or not there ought to be priority setting within the budgets of those agencies and cross-cutting decisions made with respect to projects funded by each of them, and the very basic problem is simply this. We have an enormous growth in the Federal research enterprise on university campuses and other places, and that growth is very encouraging, I think that is a positive sign, but the growth has produced a very large number of requests for funding of specific projects, and that large number of requests for, in most cases, meritorious projects has outstripped the ability of our Federal agencies to respond.

We have generally increasing Federal research budgets even at a time of decline in other domestic spending for many domestic programs, but even that growth in spending for Federal civilian research is outstripped by the size and increase of the research force, and so we are forced to examine the need for better priority setting, and it is that problem that brings us to this series of hearings.

So I hope that explanation gives a little clearer cast to what we are about here, and that is in response to your question, and I thank you for raising it because it gives us an opportunity to clarify what, certainly to you and perhaps to others, may have been some doubt as to what our mission is.

Having said that, let me get you to respond to what has been stated to us by virtually all witnesses as that clear need. Do you agree that that problem exists? and, if you agree that it does, how do we respond to it other than through examining the priority setting process? Is there some other approach we ought to be taking?

Dr. BROOKS. Mr. Chairman, I fully agree that the problem exists, and I think you have formulated it extremely well. I think the reason I raised the question and the reason I was confused about the hearing really has to do with the second part of my comment—namely, it seems to me that suggests that something more has to be involved in the priority setting than choosing between disciplines.

As I indicated, if you take the whole Federal civilian research spectrum, most of that, at least two-thirds, of that $30 billion is driven explicitly by particular societal needs or societal goals. In fact, Professor Dutton has given one example of that and hypothesized a particular goal.

But I don't think the choice is only between disciplines; it is also a choice between goals; and I think sort of in much of the discussion there has been considerable confusion between choices among disciplines and choices among goals.

Mr. BOUCHER. Well, let me stop you with that.

Dr. BROOKS. That is what makes the thing so complicated.

Mr. BOUCHER. I think that is a very valid question for us to ask. Do we have today some explicit set of criteria by which choices with regard to Federal research funding are made? And, if we do not, how would you recommend that we go about establishing that kind of criteria, and where should that be done? Should that be done externally within the research community? Should it be done by the executive branch through some process, or is that a province that should reside here in the Congress?

Dr. BROOKS. I guess my quick answer off the top of my head would be all of the above. I think it is inevitably, and this is really—in my detailed testimony I tried to set this out in considerable detail—this is bound to be both an interactive and an iterative process in which both the scientific community and the political community, both through the administration and the Congress, has to be involved.

The Executive Branch has to be involved because it seems to me they have to propose the goals. Congress can't propose the goals, but Congress can modify and tailor the goals and must do so in the light of public opinion, in the light of their very great information gathering capacities, and so on. So you are dealing with a very, very complex system, and I don't think the function of priority setting can repose exclusively in any one of those institutions.

Mr. BOUCHER. Okay. That answers very nicely the second part of the question. Let me go back to the first part, which is, how about the adequacy of our current explicit criteria? Are there any? and, if there are any, are they adequate?

Dr. BROOKS. I think the criteria that we are using at present are not explicit enough, and I think Ralph Gomory gave some very good examples of how the lack of any clear statement of goals leads to a lack of criteria for choice. That, I think, particularly applies to the mega-projects, although I think it does, to some extent, apply across the board.

I was very interested in Professor Dutton's statement of the goal that was assumed by the Space Science Board task force. I have never seen that so explicitly stated before. I think that is a very good statement, but I would be very surprised if more than 10 percent of the American public would agree with that statement, and I think that illustrates one of the problems we have. Certainly NASA would not agree with that statement, at least at the top levels, and I think that has been one of the problems of the space program all along.

Mr. BOUCHER. Let me sort of embark with you in a different direction, and I'm going to turn to the other panelists here momentarily, but because your statement was perhaps the most pointed in terms of, perhaps we don't need to make major changes, I'm beginning this inquiry with you.

What you said, as I understand it, is that you think we ought to have a better system of setting goals.

Dr. BROOKS. Yes.

Mr. BOUCHER. And I think we all would agree that that is the case.

Let's say that somehow we can establish effective collaboration with the external community and the administration and the Congress and reach that goal. Let's assume that can happen. That is a big leap, but let's suppose it does.

Let me then move to the next step, which is, having set a better set of explicit criteria by which decisions for Federal research funding are made, what then about a mechanism that would enable us to execute those goals? We have been focusing on possible mechanisms. Do you, Dr. Brooks, agree that a better mechanism that we presently have would be necessary? and, if so, do you have any suggestions with regard to the kinds of things we have talked about so far, which I think you have reviewed as a part of your preparation for this hearing?

Dr. BROOKS. Well, I must confess, I don't have the complete blueprint in my mind. I think we need to look at some of the successes and nonsuccesses in the mechanisms we already have and try to see what we can learn from them.

I have cited the example of the FCCSET's Global Change Program analysis. I think that is an example of quite a successful mechanism. I think we can go back to the period that I remember a lot better because I was more intimately involved in the 1960's when the Committee on Science and Public Policy, of which I was at that time Chairman, of the National Academy of Sciences, conducted a series of studies of the various scientific disciplines—these, in some cases, have been followed up with subsequent studies—in an effort to look at priorities primarily within disciplines, because we had a different set of panels for each discipline.

We learned a good deal from that series of exercises. I tried to summarize what we learned in an article that was published in Daedalus in 1978 and which I submitted for the record to this subcommittee.

We found it extremely difficult to set priorities between disciplines for the reason that if there is no real scientific connection or very little scientific connection between disciplines, it is very difficult to decide how to allocate resources between them because that is not a scientific question and scientists have no particular comparative advantage in addressing that question.

Mr. BOUCHER. Well, I think your statement perhaps suggests that some mechanism to do precisely that is needed.

Let me ask this question of the entire panel, if I may. One of the things that emerged from our last hearing was a suggestion that perhaps, somewhat in accordance with what you are recommending, Dr. Brooks, that FCCSET be relied upon as the over-arching mechanism for establishing priorities and that that is the level at which cross-cutting decisions among agencies ought to be made, that FCCSET, to inform itself, should request and set up some procedure by which individual disciplines set priorities internally.

We have examples in astronomy and ecology where that has happened, apparently with some success, and one question is: What about a formalization of that kind of procedure by which other disciplines are asked to do the same and make recommendations with regard to how funding ought to be prioritized internally within that discipline?

One question is: Can that be done? Obviously, in the field of astronomy, where you are dealing with ground-based instruments, there may be a somewhat greater ease in establishing those priorities than in a somewhat more amorphous field, like physics or chemistry, generally. How about those broader fields? Can we reliably expect that priorities can be set by those who are engaged in research within them? And so what about that general proposal that we formalize within FCCSET cross-cutting responsibilities and, in turn, ask that internally within disciplines those engaged in those disciplines make recommendations for priorities?

One particular question I have in that regard would be directed to Governor Celeste, and that is: Does that involve a sufficient amount of outreach? You had talked about the need for outreach in your testimony. Does that do it? If not, do you have other recommendations for how additional kinds of outreach should be sought and also incorporated into that process?

Well, we will begin with whoever wants to start.

Governor Celeste, would you like to begin?

Mr. CELESTE. Thank you, Mr. Chairman.

Could I drop back just a little bit to the comment earlier, because it sparked a thought in my mind.

I want to agree very much with Ralph Gomory. I don't think that the goals are at all clear for where we want to get to, and priority setting really requires that, we have to state them very clearly, and building a consensus around that is one place where public participation can be heard so that you can get it at the front end around the goal setting discussion.

There is a difference between goals and criteria, and I think that in the decisions about what we fund at the individual investigator level, the criteria are reasonably well established, and they are pretty simple. We look for excellence in research; the quality of the proposal is vital; and, secondly, we look for promise: Is it going to lead to new knowledge down the line? And I'm a little uncomfortable with how we get disciplines to set priorities and then get them adopted by FCCSET and what that means to the individual investigators, particularly—and this is a question that I've wrestled with—how do we ensure an investment in what I would call the offbeat, the work that challenges conventional wisdom, the work that says do we need to test this accepted notion in some profound way.

So I think that however we design that process that invites disciplines to say yes, we do have some priorities, that there needs to be room within that process to encourage creative, original, sometimes controversial research at the most basic level, and I would again reiterate something that Dr. Gomory said, that is, the place where this Nation is the recognized leader in science is where we have generated enormous research capability at the individual and team level in the universities and research labs and so on.

The issue, it seems to me, is how do we bring this system into focus where there are meaningful trade-offs to be made, and that is not at the level of individual investigators, it is at the level of, let's say, do we do one more mega-project or do we invest in infrastructure in some very substantial way, because if we are going to make a $10 billion commitment to this long-term mega-project, perhaps that money is better spent in investment in research infrastructure that serves the total system. How do we get at that kind of consideration? and it seems to be that being very clear about our goals and then weighing the decision at a FCCSET level with, I would say, some real opportunity for public discussion, but part of the problem right now is, we arrive at a research agenda after the fact. It is really the OMB process, by and large, with some very skillful agency work that dominates, and then it is congressional decisions, and so we look at the agenda after it has been developed, and somehow or another we need to invite public participation.

I would add that that is increasingly important because the research enterprise is diffuse in other ways. If States are now spending a billion dollars on science and technology—and I would guess States are spending somewhere between three-quarters of a billion and a billion dollars—a substantial portion of that, let's say 20 or 30 percent, reasonably focused on basic research rather than simply technology spin-offs and so on, that is a significant investment on the margin, and somehow or another that needs to be taken into account in the process of arriving at policy priorities and in the consideration of what kind of leverage can we get for our resources, where do we want to encourage cooperation as a result because we can do more through the cooperative effort than through a range of competitive activities across the board.

So I'm perplexed as to how, within the discipline—how far you go in setting priorities and making a meaningful contribution that rises to the FCCSET level. I do think that there is merit in that forum, that mechanism, for bringing Federal agency heads together, coincidentally, I think, often very unclear about their goals, maybe even deliberately so, because if their goals were very clear, then all of us would challenge them more energetically.

I don't know whether that has been helpful.

Mr. BOUCHER. Well, no, it is; it is extremely helpful.

What I gather from your comment is that you don't see a particular problem at the individual investigator level or perhaps even within disciplines in terms of priority setting; the real problem is higher up the line; it involves setting priorities among agencies and among larger programs and that really our focus ought to be more there than internally within disciplines at all.

Let me just ask this. Is there some value in attempting to get other disciplines to do what the astronomers have done, for example? Is that a helpful or a harmful process?

Mr. CELESTE. I think that it would be helpful if it were couched particularly in terms of the opportunities for cross-discipline work, because that is, it seems to me, the place where you could raise up quite legitimately something that is missing today. More and more of the exciting work in science is being done across disciplines, and if one were to invite those within a discipline as a means of challenging them to think about priorities—what are the opportunities for cross-discipline work that you can identify within your discipline, as you think about it? who do you need to reach out to?—then what you have done is to identify a level of activity which reaches beyond the competence of one discipline to kind of engage and make it happen.

Mr. BOUCHER. Okay.

Dr. Gomory.

Dr. GOMORY. Thank you very much.

I would ike to reinforce the remarks of several of the speakers on the importance of setting goals. First of all, I think it is the appropriate thing for this to happen outside of science. In other words, I think for you gentlemen to decide on how much effort we want to make toward a clean environment versus how much effort we want to make in having world class physics versus certain other things, that is a layman's decision, that is a value decision. If those things are articulated, believe me, the rest of the process is doable. I really don't think it is as hard as people think, and let me try and explain how, and I claim to be a veteran of this process, okay? so it is not a completely naive viewpoint.

Let's take the scientific subjects. If you don't set some kind of a criterion for them, it doesn't matter how much you will spend; any budget you can spend to it; it wouldn't matter if you tripled it. I mean it would take a few years to catch up, but don't worry, we'd spend every penny of it. And that is true of anything. You could spend an infinite amount of money on the environment, and in my past history we could have spent an infinite amount of money on memory, we could have spent an infinite amount of money on displays. So it is not a problem which is solved by anything except setting some criterion.

I suggest that for the scientific subjects where we have not had a goal, and, indeed, in most of these subjects we do not, that we set the goal of being world class. That is a relatively measurable thing; you can compare with other countries; you can make a rationale for it, because today science is international, and therefore we do not march a branch of physics forward alone. There was a day when that was true; that day is past.

But, if we are not world class, the tremendous generation of knowledge is something we will not be able to absorb as well as generate. In other words, you can't benefit—and historically there has been a great deal of benefit—from the advances of basic research unless you are a first-class participant in it. There is no such thing as simply absorbing foreign science; it doesn't work.

So I think if we set as a goal to be first class in these fields, we will get the proper national benefits. Also it's a measurable sort of thing; you can say, "Is the United States world class in physics?" Now once you have decided to do that, you get into the question, well, now, how do we do that within physics? That is a doable thing, and it is less delicate than the word "priority" would seem to suggest. "Priority" always seems to suggest, well, we are going to put down a list, okay? and the best things are going to be at the top and we're going to choose them, and the things at the bottom will follow. That has not been my experience.

If you want to have a first rate physics program, there are probably a lot of different ways to have it, and it may not matter all that much exactly how you do it as long as you end up with that result. There were eight different displays programs that we could have had, and what mattered was that we make sensible choices and end up with a decent display program.

So the notion that you really decide by ordering everything, which is an extremely difficult process, and then picking the things off the top doesn't work, because you will end up with an incoherent set of things, if you see what I mean. The set of things at the top are not the set you want to end up with if you want to have a fairly reasonably balanced physics program.

I really do think—and therefore this is not as difficult and not as sensitive as one might imagine. I really think that the subcommittee and others would make a tremendous contribution by articulating goals. I'm sure that the various mechanisms, including FCCSET—though your point that FCCSET needs an outreach beyond the Government is absolutely correct, because much science is done that is not governmental. I think we could come back, using various mechanisms, with a very reasonable program for physics and its subfields, and I think it would be one that would make us world class. I think we could do that for every field. I don't think the bill would bankrupt us at all.

So I think the thing to focus on is, what are we aiming at? and if we can articulate that—and I believe that also is doable—I think we can get on from there, and that, of course, I think, puts in the hands of laymen that which is truly a value judgment.

Mr. BOUCHER. Well, that is an intriguing recommendation. I take it you do not mean that we should simply as a Congress say that it is our intent to become world class in every field. That is essentially what we are trying to do now; we are trying to capitalize on every opportunity that comes along and do it today, and our budget doesn't accommodate it, and that is what brings us to this set of questions.

Dr. GOMORY. If I may respond.

Mr. BOUCHER. Please.

Dr. GOMORY. Being world class in every field is, in my opinion, doable. Responding to every opportunity is endless and not doable. You can be world class by responding to a subset.

Mr. BOUCHER. Where does the decision then get made, assuming the attempt to be world class in every field, in choosing among the various projects that will take you to that goal, where is that decision made?

Dr. GOMORY. I would say that the best of the existing mechanisms would be OSTP and FCCSET, because they have a reasonable overview, and if you then inform that by outside input, that you would have the basis for a judgment and comparison with other countries, which is implicit in it.

Mr. BOUCHER. Okay. That brings us right to the point then. So some improvement in the FCCSET process and some institutionalization of what it is attempting to do informally now would, in your view, be helpful.

Dr. GOMORY. Absolutely.

Mr. BOUCHER. All right.

Dr. Dutton.

Dr. DUTTON. Mr. Chairman, I would like to make a few comments on this—

Mr. BOUCHER. Could you move the microphone over please, sir?

Dr. DUTTON. Thank you for reminding me.

What we are trying to do here is decide the answers, really, to two key questions: What should we do? and, How should we do it? and I think the question of, What should we do? needs to be broadened into two questions in order to make this effective. The first is: What do we need to do? and the second is: What can we do? and I'm going to assume that we do have a clear understanding of national goals in what we are trying to accomplish, and I think this panel is—I'm pleased to see is unanimous on the need for that.

If we do have that understanding, we can start to talk about strategic endeavors. Now first, and a key strategy is to support fundamental research. Basic research really provides the foundation for scientific progress. It arises from various forms of curiosity, and we need to manage our basic research program very carefully but not too closely, and I think that one of the things we realized in our work with NASA five, seven, years ago, is that it is almost impossible to set priorities between disciplines; we need to work on setting priorities between initiatives and special endeavors.

You know as well as I do that in 1990 we spent about $11 billion on basic research, Federal support for basic research, some 17 percent of that $70 billion, and we need to first decide what fraction of the Federal budget we are going to spend on basic research. I think we can manage that, and I think we can stimulate creativity of the kind that has been successful, and we can then turn our attention to other strategic endeavors that should focus on compelling scientific, technological, economic policy issues and on areas of unusual accomplishments.

Let me suggest a little different process than the one that appeared in your last hearings. In one direction, we need to complete a high-level assessment of scientific opportunities and national needs. Now this effort could be managed by OSTP and FCCSET, maybe in cooperation with OTA, and I would think it should involve key scientists, representatives of business, policy makers, and this effort would set the stage for deciding on strategic endeavors; this is the top-down part of the process. Second, proposals for promising initiatives in science and technology could be generated and ranked by the National Research Council, for example, in a comprehensive process that involved all its disciplines and its interdisciplinary boards and committees, so the disciplinary agenda such as that done by the astronomy community would flow upward through a process managed by the NRC to formulate a preliminary national science agenda.

Again, clear criteria must be formulated in advance; proposals must be written specifically to answer those criteria; and, as I said, we are trying to figure out what those criteria should be in our task group.

Given those two assessments, a joint commission embodying the leadership of the OSTP, FCCSET effort, and the NRC effort could combine the assessment of opportunities and needs and the highest priority scientific initiatives into a comprehensive agenda perhaps for a decade. The result would be six to 10 strategic initiatives for the coming decade and a number of separate scientific initiatives. Perhaps these initiatives could be incorporated as a special section of the President's budget for examination by the Congress. Presumably they would be formulated, as is the U.S. global change research project, so that they could be managed in concert by the various agencies.

I would like to emphasize, though, that the agenda should distinguish carefully between scientific and technological endeavors. They are too often confused. Science develops an understanding of the relations between causes and consequences in the physical and biological world; technology provides processes or devices that allow us to do new things or to perform more effectively.

Certainly science and technology are intertwined and stimulate each other, but by recognizing their difference we maintain an appropriate balance and ensure that both will advance at a suitable rate.

Thank you, Mr. Chairman.

Mr. BOUCHER. Thank you very much, Dr. Dutton. That is an interesting recommendation.

I am particularly intrigued by your suggestion that the National Research Council play a role in coordinating advice from the outside, whether that advice come from individual disciplines or from other sources.

Governor Celeste, would you care to comment on whether that kind of approach might satisfy your suggestion that we have a broader outreach effort and obtain more outside advice?

Mr. CELESTE. I think that it would certainly provide an opportunity for the laborers in the vineyard to be heard, those who are doing the research at the lab level and others, although even there I think there is a challenge to hear what I would call minority voices, sometimes literally minorities but young investigators, those who don't have established reputations, and so on, or those who may be challenging conventional wisdom. But I think that typically the NRC would be best at hearing from the research community, let's say, and the issue that I would suggest that needs to be dealt with at some point is, how do you get the broader public engaged in this process.

I actually am very attracted to Dr. Dutton's notion that, as a nation, we would be well served if we could identify a half-dozen large—genuine priorities, national priorities, that we are prepared to devote a maximum effort to over a decade or so and then regularly we come back to that agenda and evaluate it and revise it on some regular basis. I think that requires a public outreach that somehow goes beyond what we would normally think of as the community the NRC is in touch with, whether that—and these days there are a variety of ways to do that. I mean you can literally have electronic town meetings, you can have—as Members of Congress, I don't have to tell you the numbers of ways in which you can engage people in a discussion of serious topics, and there may well be an occasion that one could do regional hearings in an appropriate time frame to allow both public input and expressions of interest and concern and a level of public understanding.

Mr. BOUCHER. Let me inquire finally of this panel a question suggested by that answer, and that is this. I hear Dr. Dutton recommending that what we probably ought to look at is a number of—and only a handful at that—of major Federal priorities that then other research would flow from, and five or six is the number suggested. Are we not, in a sense, doing that now with the grand challenges or the grand initiatives that the FCCSET process has come forward with in biotechnology, in high-performance computing, in critical materials, and other areas, and if we are, in fact, doing that today, does that meet the need? What are the shortcomings of what is currently happening, and why do those grand initiatives not meet the test of what you set forth, Dr. Dutton?

Dr. DUTTON. Well, I think that that is the sort of thing I have in mind. I don't think that they were arrived at by as public a process or as broad a process as I suggested in my version of the FCCSET process that went into the community and did some of the things that Governor Celeste mentioned.

The other part of that that is missing is that the flow up through the disciplines of opportunities in science has not been—I don't think has been formally coupled to create an agenda.

Mr. BOUCHER. Dr. Brooks, we haven't heard from you. Let me get your comments.

Dr. BROOKS. Yes. I generally agree with what has been said. The point that I was really trying to make in my critique was that disciplines are not the only dimension on which you can set priorities. I don't think you can organize the whole priority setting process simply in terms of disciplines. In fact, all of the examples that you cited of the FCCSET initiatives are not disciplinary initiatives, they are initiatives that are mixed, technical and social.

The other point—just to amplify one of the points that Governor Celeste made, I think one of the shortcomings of the peer review process that I have observed is that it tends to, I think, too frequently reject interdisciplinary initiatives, and, in fact, you need a somewhat higher level process to produce interdisciplinary initiatives than the normal operation of the peer review process in NIH and NSF.

The symptom of that is, especially in times of shortage of funds such as we have now, a project that involves two different divisions or two different programs in NSF has a terrible time getting funded. You will get a very—what will happen, because I have had this experience myself—you will get a glowing review from one division and a totally negative review from another division and still another one saying, "Well, this is a very interesting project, but this doesn't fit into our division," and what you end up with is negotiating with two or three different parts of NSF and essentially almost yourself having to put together a coalition of sponsors in order to get the project funded. Now if you happen to be somebody with a long track record and well known name and so on, you can do that, but if you are a young investigator, that process is a nonstarter.

Now the NSF does have some programs like the Waterman Award and so on that do help that a little bit, but I do think we have to invent better ways of allowing for interdisciplinary initiatives, because some of the biggest innovations, in fact, occur at the boundaries between disciplines rather than in the disciplines themselves.

Another thing you have to remember about disciplines is that this year's physics will be next year's chemistry and the year after next's biology. The disciplines are like clouds; you know, the drops in the cloud are never the same from one minute to the next, but the cloud always looks more or less the same. But the discipline—and the content of the disciplines is constantly changing and borrowing from other disciplines, and I think our system has to recognize that.

Part of the problem, I think, that I see with the peer review process as it is now operating is, as the competition gets fiercer and fiercer for funding, people think of more and more reasons for turning down proposals, and very often those—and also they get postponed longer and longer, so that by the time you get funded you have almost forgotten what it was you were going to do, and that is a long subject that I can't get into, but I think the whole peer review process needs a much closer study and evaluation than it has had. I think it is fundamentally right. I think that everybody in the world thinks that the U.S. peer review process has been responsible for the world leadership of U.S. science, but, you know, there is nothing that fails like success, and you get a good formula—and General Motors is an example of this—you get a good formula, and all of a sudden it gets out of date, and none of the people involved in the process really recognize it.

So I think you really have to examine the strengths and the weaknesses of the peer review process and try to shore it up and have it a little less bureaucratic and with a little bit more flexibility and more other mechanisms.

Mr. Boucher. Well, thank you, Dr. Brooks.

Dr. Gomory.

Dr. Gomory. I'm very much in sympathy with what Dr. Dutton said about an outreach and the NRC and also with many of the things that Harvey has been saying. I would just like to add two points.

Mr. Boucher. And if you could use the microphone.

Dr. Gomory. Oh, I'm sorry. Yes.

I would just like to add to the remarks of Dr. Dutton and Harvey Brooks just a couple of points, but I do think especially the first is significant. I do think we do have goals for certain areas, and, as Dr. Dutton said, those areas were arbitrarily chosen. I think we could do better if we chose them in a more open and more systematic way.

Second, we don't have goals for science, and I think that instability which is generated by that is very much at the heart of the present difficulties within the scientific subjects themselves, because there are some people who think the thing should grow indefinitely; there is just no consensus on where are we going with the scientific subjects themselves which are the root of much progress.

So I do think settling on a criterion of being world class—which is not infinite, it is just about to be the class area—would be a stabilizing thought. So I think that that should be included. And, finally, I think we should be very careful not to overprioritize. As I tried to say earlier, you can have many different good programs that would all be quite adequate for doing anything sensible, and I think, therefore, if we do get some kind of an NRC process, it should have in it some very indefinite blobs, which is, oh, we are going to spend a certain amount of money for investigators and solid state physics, and let's not try and prioritize that sub-blob to death because it will just kill the sort of initiative that actually is quite successful.

So we are going to have to have some restraint. We will have to prioritize one half-billion-dollar project against another one, but we don't have to prioritize everything.

Mr. BOUCHER. All right. Very good.

Gentlemen, thank you for those answers. You have informed us greatly.

The gentleman from California.

Mr. PACKARD. Thank you, Mr. Chairman. This has been a very interesting discussion.

I don't think there is any argument from members of the committee and, for that matter, Members of Congress generally, that goals are not only extremely important but essential in developing our—the paths of the future and that we don't have clear goals—I tend to agree with Dr. Gomory—and it would certainly be wonderful if we could become world class leaders in virtually every discipline and every area. But the fact remains that the system that we live in, and certainly the way the system is now working here at the congressional level anyway, is that the budget drives our priorities and our goals, and I think that has been discussed, and I think the real question is—and we have only touched upon it—is, how do you change from being goal driven to being budget driven? And that is no small task as we grapple with tight and difficult budget times.

Also, I think that we are living at a time when we are seeing the globe changing and the world changing and thus goals change and priorities change, and we have never seen, I think, that process more than we are seeing right now and have been seeing for the last two years or more. And thus, how do we make those changes?

So there are two huge changes, as I see, that we are discussing. One is, how do we change from a budget-driven process to a goal-driven process? And the second is, how do we adapt that process into the rapidly changing world that we live in and, thus, the changing of goals? And I don't think there is any question that that has been taking place. And one almost works against the other, because as you become goal driven and then, according to Dr. Gomory, there will be the funds there to meet those goals, and that is not easy to see really under—especially when we deal with rather finite committee assignments and each committee has charge of specific areas of goals in Congress and our commitment to dollars. It becomes very difficult as we compete, committee to committee, for those dollars and, thus, be able to address the goals and not the dollar-driven process. And then the rapidity of the change that is taking place frustrates that process as we try to move toward a goal-oriented system because then people become frustrated because your current goals are no longer the goals of tomorrow, and thus the dollars seem to not be able to fit, and it all becomes a very, very difficult and confusing process.

A good example of that is what we are grappling with right now. And I would be very interested in your input on this—on how this transition can best take place. Much of the science and much of the research that has been done in the past, at least Federally supported research and technology development, has been done through the defense budget; probably over 50 percent of our research has been done—and development, has been done through defense dollars, and maybe significantly over; some are saying up to as much as 70 percent. And now we are seeing where defense dollars are very, very actively being debated in terms of reducing our commitment to defense dollars.

One of my concerns—and I know it is the concern of the chairman of the subcommittee and certainly the chairman of the full committee, George Brown—is how do we transfer from defense research to the private sector or to nondefense research commitments in the Federal Government. And that is not going to be a small task, without almost inherently seeing a reduction in commitment of Federal dollars to research, because as we make this transition from defense-related research to nondefense-related research and technology development—and I would be very interested in your comments on how we could best make that transition as we reduce defense dollars.

I personally foresee, and I hope I'm wrong, but I foresee where, out of our zeal to cut back defense dollars—and I'll be supportive of cutting back defense spending—we may automatically bring about a cutback in research and technology development dollars simply because we do not find a way, an adequate way, to reprogram defense research. I'm not talking about defense hardware at all, I'm not talking about defense preparation or manpower or bases or anything like that, I'm simply talking about the research component of our defense budget, and I foresee that we are going to lessen our dollars commitment to research and development rather than increase because science, space, and technology and the private or the nondefense sector of our research area in government is simply going to be neglected and not be able to receive an additional amount of these defense dollars.

That is a whole new area that I would be very interested in your comments, and let's start with the governor, and then we will come to you, Dr. Gomory.

Mr. CELESTE. Mr. Chairman, Congressman Packard, I think that your question about the redirection of defense dollars goes right at the point that you made about moving from a budget-driven to a goal-driven notion of where we invest in science. If one thinks of it as budget driven, there has been defense over here, and, in fact, it has been exacerbated because there is now a fire wall, so that is really defense, and it is not on the same table as we think about issues of investment in science.

If one—and the fact is, we had an overriding goal. It was articulated in the late forties, and which has been reflected in that level of investment—namely, to defend the Free World and to fight communism, really, wherever it needed to be fought. So we made that investment. That was a goal that everyone agreed to, and it was one of the few goals that was clear.

What we need to do now is to articulate what are the goals for a post-Cold War U.S., what does this mean for our science, and if one accepted Dr. Gomory's proposition, for example, that the basic goal that would be articulated for the scientific disciplines would be to achieve world—or sustain world-class status, then the issue is, what does it take by way of investment in that to accomplish it, and, in fact, it may only be a relatively small portion of what was being spent on defense R&D.

Now if one also sets as a goal to be a national leader in advanced materials or new methods of computation or whatever, there are a series of investments that follow those decisions as well, but it seems to me that being very clear about the goals for the next decade is a way to move us beyond the constraints of the budget. It also helps in two other respects. Looking at it as a former governor, it would have been extremely helpful to me in Ohio where we were investing, let's say, 20 or 30 million dollars, not big money from congressional standards but big money for the State of Ohio, real time dollars that were going into basic research.

If I knew that the presidents of the research universities and the others who were involved in the decision-making process could make their decisions within the larger context of some national priorities, it would have helped us. We would have said, okay, let's choose where we can be world class in these particular arenas that are now national priority arenas, and we would have had much more confidence that those investments were wise, and it would have leveraged dollars.

Finally, I think there is a very big issue that needs to be wrestled with by Congress around the mega-projects that involve hardware. I personally believe that those ought to be, to the greatest degree possible, international investments. It is very hard to imagine that the investment we make in any mega-science project, whether it is the SSC or the humane genome, is not going to benefit scientific research globally, and therefore, somehow or another, I would suggest that once we set these goals—and, again, being world class, being world leader is part of it—we should not be afraid of or fear international participation, we should identify the places where we welcome it, invite it, and encourage it, and nurture it as part of the process.

Mr. PACKARD. Before we go to Dr. Gomory on my first comments and question, let me respond to that a little bit, Governor. Tomorrow, we will have on the Floor of the House a debate on the space budget. I think we are having a vote this week. One of the major parts of the debate and one of the amendments that will be proposed is that we discontinue the space station, one of our first, at least, and certainly the most major international project that has come out of this committee.

The international community is very, very tenuous about their commitment to join with the U.S., and that lack of commitment will be exacerbated significantly if, in fact, we pull the rug out from under one of the major international projects where our international partners have already gone down the road significantly with us. That is why perhaps we are having difficulty getting international partners on the SSC, and there will be probably—and if we are not successful in maintaining the space station as a part of our long-term goals in the future, then I would almost say that international cooperation has gone at least for the immediate future; we would have lost significant credibility.

I would be interested in your comments on how we can develop that credibility other than simply by maintaining—retaining our original contractual agreements with our international partners.

Mr. CELESTE. When I was working for the Foreign Service in India back in the 1960's, I remember an expression, "We want to be in on the take-offs as well as the landings." I think part of the challenge is, how do you engage international partners in the discussion in an early stage of a major scientific initiative. We don't have good tools developed for very early consultation and participation in the decision to make this investment on this scale at this point in time.

We have been talking about how do you get participation just in the formulation of national priorities, so that is a very tough problem, but I would say part of the challenge is to maintain credibility with international participants. Part of it is to formulate vehicles that provide an opportunity for them to have a sense of ownership from the enterprise—of the enterprise from early on, and I don't have a good suggestion to make there, other than I think that that is a key ingredient to somehow making it happen.

We had some of the same problems simply getting interstate cooperation around initiatives where it didn't belong to any one State, and the question was, if one State said, "Okay, this is what we think is a good idea; everyone come on board," it was very hard to get my colleague governors interested, but if we could sit around and say, "Let's talk about four or five different problems," and from that identify one that we wanted to work on together and really go at it jointly, we came up with some very interesting initiatives, and actually we had governors put money on the table to support joint initiatives as a consequence.

Mr. PACKARD. Let's go back the original point, though, and that is, how do we make the transition from a defense-related research program to a nondefense?

Dr. GOMORY. Mr. Chairman, Mr. Packard, I think these are tremendously pertinent questions, and I will certainly do my best to reply. The difficulty that you describe about the fragmentation of the jurisdictions of the committee is, of course, an enormous one.

I think the only way that I can imagine that that could be dealt with is if the administration brings forward a program which it can characterize as providing goals, providing a program that meets goals—for example, that it does make you first rate in physics—and of the pieces of that, part of it would be in the Defense Department, part of it would be in the Department of Energy in the form of accelerators, and part of it would be somewhere else. But they would have to provide—I can't imagine how the diverse committees could, from the bottom up, generate that coherent thing, but they could look at it and say, "Gee, this piece in my committee is convincing; I'm going to defense it."

So it seems to me that you have to start with a map and you have to critique your part of it. It is not plausible to imagine the committees can generate the map from the parts.

So I think that that is where the mechanisms suggested here would play a role in the FCCSET, the OSTP, and the outreach through the NRC. They would have to generate a program that added up, and then you would have to deal with the parts of it and see if you find it convincing.

I would say parenthetically that the goal of being first rate is not an infinite goal, but were it, were it to exceed the budget limitations, then you simply have to decide which parts of it you are not going to do, and that is normal life.

With respect to the defense dollars, I don't have an answer at this point, but, again, had we the mechanism described, then the defense budget would be coming to you with certain parts earmarked: "This is the part we need to keep our solid state physics healthy in the country," or possibly with the remark that, "This solid state physics support has now been moved to another agency, and we suggest that the dollars should be moved." But they have to give you the map, in my opinion.

And, finally, with respect to your second point about international cooperation, I think that the U.S. instability as a partner is partly inevitable and partly not. It is inevitable because we are a democratic society, and what we decide in a certain year does not really bind us very effectively if our views really change. But I don't think that the space station is an example of that. In my opinion, the space station is an example of our not being clear from the beginning, and also Harvey's point, we didn't get the other fellows in at the beginning either.

What is it for? Is it that we have a national goal which the people are behind to settle space with people? Is it supposed to be first-rate science? You see, it is an amalgam, and it is shaky, and if you start with unclear goals, then it is much more difficult to have a lasting commitment.

Mr. PACKARD. Thank you very much.

Dr. Brooks.

Dr. BROOKS. Yes. I think that one of the things that needs to be done on the defense question—you know, a great deal of the defense R&D budget is highly specific to defense and has really only very general impact on the civilian economy, and I think one needs to really analyze the defense R&D budget very carefully, and I think the administration has got to do this—this can't be done by the Congress—and look at those things that defense has traditionally done which don't tend to get done otherwise. One of them certainly is very high risk, long-term R&D, technology investments.

But the long-term part of those investments tends to be relatively cheap. You know, only 3 percent of the defense R&D budget is what is called 6.1, which is more or less the equivalent of basic research, and even 6.1—I guess 6.1 and 6.2 together is only between 2 and 3 percent, and that is where most of the value for the civilian economy comes from out of the defense budget.

Now when you are spending 10 or 15 times as much money as that on development, there is a good deal of what I would call bootlegging of basic and exploratory research that is just sort of the leakage or the uncertainty in the development budget, but still it is a small part of the total defense R&D budget.

So I think one needs to—maybe Congress could make a request of the administration to mount a study of this. One needs to look at the defense R&D budget and look at those things that are really important—potentially important for the civilian economy that don't tend to be judged by the same criteria on the civilian side of the Federal defense.

I have a feeling we are willing to take much bigger technical risks in the defense side of the technology and science budget than we are in the civilian side. So we at least begin—ought to begin to be applying similar criteria on the two sides of the fence.

Mr. PACKARD. Thank you.

Dr. Dutton, do you have any comments?

Dr. DUTTON. We have really two topics on the table at the moment, one international cooperation and the other issue of the Defense Department or defense budget.

In international cooperation, I think we have often gotten ourselves into an interesting contradiction that our goal is to maintain U.S. leadership and we want our partners to help pay for it, and you can't have that one both ways, and so I think that what my fellow panelists have been saying is that if these projects had been started earlier, some of these projects had been started earlier, as this is an exciting area of science or technology and we are inviting other nations to talk with us about what part they might contribute in a joint effort. We would find out whether it is a project worth undertaking on an international basis, and we would have a much stronger basis for going ahead.[1]

When we have large endeavors that we attempt to get other people to invest in and they won't invest, that may be a sign. Certainly the business community knows what to do in that case.

Relative to the defense budget, I have two comments. As my fellow panelists have said, if we had a national agenda and if we had goals, clear goals, it certainly would be appropriate for certain parts of that agenda to be managed by the research management establishment in the Department of Defense and that that would be very appropriate.

There are some very interesting scientific problems today that one could argue are most appropriately managed by the Department of Defense.

Second of all, in the era that we are going into, it seems to me that it is very important that we continue our investment in fundamental research and knowledge that is related to what we are going to need for defense needs in the future at the same time that we abandon spending as much as we do on hardware and specific systems and so on. We are going to need to maintain our knowledge of technology, science, and those sorts of things that are defense related even though we do perhaps scale back on machines and people in the Defense Department at this point. I think we need to consider that very carefully.

Mr. PACKARD. Thank you very much, Mr. Chairman. Thank you.

Mr. BOUCHER. Thank you, Mr. Packard, and, again, the subcommittee expresses its appreciation to this panel of witnesses. Your

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[1] In international cooperation, I think we have gotten ourselves into an interesting contradiction: we say our goal is to maintain U.S. leadership and yet we want our international partners to help pay for it. We cannot have it both ways. I think that what my fellow panelists have been saying is that if we would consult our partners earlier, if we were to say this is an exciting area of science or technology and we are inviting other nations to talk with us about what part they might contribute in a joint effort, then we would find out whether a project is worth undertaking on an international basis, and we would have a much stronger basis for going ahead.

expertise has been of great use to us this morning, and we appreciate your taking the time to share your views on this range of important issues with our membership.

We may have some follow-up questions for you, and after we have formulated some recommendations in this area we will probably call on you to comment on them, and your further advice will be most welcome.

There being no further business to come before the subcommittee, this hearing is adjourned.

[Whereupon, at 11:24 a.m., the subcommittee was adjourned.]

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